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LECTURES

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BY

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Fistula in ano. Ulceration of the intestinal mucous membrane. Cases of ulceration and perforation of the intestinal canal. Fistula in ano caused by ulceration of the rectum. Causes of the ulceration. Symptoms of abscess near the anus. Situation of the orifices. Treatment. Ward's paste. Operation.

GENTLEMEN,—I presume that you are all aware of the fact that abscesses are very liable to form in the vicinity of the rectum, and that when so formed they heal only with considerable difficulty, and, for the most part, do not heal spontaneously. You are also aware that the parietes of those abscesses contract, and become hard and callous, in which stage the disease takes the name of *fistula in ano*.

Now, this affection, although of frequent occurrence in hospital practice, is much more common in private practice, and, therefore, it is, in every point of view, a disease of great interest to the surgeon.

The first question that presents itself is this,—Why is it that abscesses are so particularly liable to form in the situation in question, and that when so formed they do not heal like abscesses occurring in other parts of the cellular membrane? I formerly supposed that the healing process was prevented chiefly by the irregular action of the sphincter and levator ani muscles. Further consideration, however, and more mature experience, have led me to the conclusion that this opinion was incorrect. That such causes may interfere with the healing of any abscess I well know, but I am now fully satisfied that they will not afford sufficient explanation why it so rarely happens that abscesses near the rectum heal spontaneously, and, at any rate, it is quite clear that the action of these muscles will not explain the formation of these abscesses. In order to explain their formation I must call attention to what happens in other parts of the intestinal canal. The mucous membrane, under a variety of circumstances, is liable to ulcerate. In patients who die from diseased liver, or phthisis pulmonalis, or at the end of continued fever, and various other diseases, you find the mucous membrane of the bowels ulcerated. This ulceration seldom extends further, does not involve the muscular tunic, but sometimes the latter is affected, and then some of the contents of the intestines escape. Should this occur where the intestine is covered by peritoneum the contents may escape into the peritoneal cavity. For example, there was a little boy, seven years of age, who had symptoms of mesenteric disease, and who had just recovered from what was supposed to be a fever. When he appeared to be convalescent he was suddenly seized one evening with what was called a fainting fit, in which his pulse was not perceptible. After some time, under the influence of a stimulant, he recovered; nevertheless, he continued low and depressed. On the following day he had another attack of the same kind, from which he did not rally, and on examining the body after death I found that there was ulceration on the inner surface of the ileum, and that the mesenteric glands were diseased. In one place the ulcer had extended by a small opening through the muscular tunic, and also through the peritoneum, and a small quantity of the feculent matter had escaped into the cavity of the belly. Every person who has had much experience of disease has seen cases of the same kind; but there are others in which both the muscular tunic and the peritoneal coat ulcerate, and yet the contents of the intestine do not escape into the cavity. Adhesions take place round the ulcerated spot before the ulceration of the peritoneal coat is completed, and these adhesions cause the contents to escape, not into the peritoneal cavity, but to become infiltrated into the cellular membrane of some part of the abdominal parietes. A young man, of seventeen or eighteen years of age, who had long been in ill-health from disease of the lungs, and who was indisposed in other ways, was supposed to be rather

better than usual, but one evening he was seized with violent pain in one side, and there was considerable tenderness of the whole of the abdomen. Two physicians were sent for, the symptoms were not exactly those of peritoneal inflammation, but they could not explain the symptoms so well in any other way as by assuming that he laboured under peritoneal inflammation. The inflammatory symptoms subsided, and two or three months afterwards I was called in to see him on account of a tumour which had formed in the front of the belly. It was an abscess; I opened it, and there came out pus, and with it a good deal of foreign matter, which I was satisfied must have come from the intestinal canal. The abscess made its way in several other places, and ultimately this young man died. On examining the body after death it was found that there were ulcerations at the lower part of the ileum; one of these ulcers had extended through the muscular and peritoneal tunics, but around that ulcer the ileum had contracted adhesions to the abdominal parietes above the groin, and the matter had escaped into the cellular membrane between the layers of the abdominal muscles, and from thence had made its way forward to the part where I opened it. This patient died, but it is not a matter of course that, under such circumstances, such should be the result. I was called in to see a little boy who had been supposed to labour under mesenteric disease, and I found that there was an abscess near the umbilicus, discharging pus and feculent matter. We attended to his general health, kept him in a recumbent position, lying on his back, which, I apprehend, was a most essential part of the treatment. Some very simple dressing was applied to the opening, and the boy ultimately recovered. I do not know whether he is still alive, but he was alive two or three years afterwards. I saw another case of the same kind, and I know that the boy lived a considerable time, but as he was taken away from London I cannot tell whether he ultimately recovered or not.

That part of the intestine in which ulceration of this kind is most likely to take place is the lower part of the ileum, but it not unfrequently occurs in the cæcum. Abscesses in the right iliac region generally belong to the cæcum. A young man, on jumping from a coach felt something, as he said, give way in the right groin, and he came to London with a tumour in that situation. I thought that there was a deep-seated gland which was suppurating, and recommended him to go home, to keep quiet, and to poultice the tumour. A month after he had jumped from the coach he sent to say that the abscess had burst. There was a discharge of pus from it, but it was of a very offensive character, and on examining it carefully I was satisfied that there were fæces mixed with it. He had no bad symptoms at first, and being a very nervous man I did not tell him the exact nature of the case lest it should alarm him. Two or three days afterwards he took some medicine, and a draught of decoction of bark. A short time after, to his horror, the decoction of bark ran from the groin and frightened him out of his wits. From that moment his nervous system began to give way, he became in a state of great nervous excitement, and died ten days after the bursting of the abscess.

On a post-mortem examination I found an ulcerated opening of the cæcum; the fæces had escaped through it into the cellular membrane at the posterior part of the cæcum, and formed an abscess, which burst into the groin. There was a woman in this hospital, with an abscess in the groin, and we supposed it to be connected with dead bone, which is the case with a great number of chronic abscesses. Perhaps sufficient attention was not paid to the quality of the discharge; but one day the woman, in taking off a poultice, found in it a lumbricus. She ultimately died, and on examining the body after death we found an ulcerated opening of the cæcum through which this intestinal worm had made its way. It was evident that the ulcerated opening of the cæcum, which was at the posterior part, had been the beginning of the abscess. Though these cases are not exceedingly rare, I mention them, because stating specific cases will often impress an important fact on the mind much more than a general observation.

I believe that this is the way in which fistulæ in ano are always formed, namely, the disease is originally an ulcer of the mucous membrane of the bowel, extending through the muscular tunic into the cellular membrane external to the intestine; and I will state my reasons for entertaining that opinion. The matter is one of great interest as a question of pathology, but it is one of great importance, as I shall show by and by, in connection with surgical practice. It is admitted by every one that in the greater number of cases of fistulæ in ano there is an inner opening to the gut as well as the outer opening; and I am satisfied that the inner opening always exists, because I scarcely ever fail to find it, now that I look for it in the proper place and seek it carefully. I have, in a dead body, examined the parts where fistulæ had existed several times, and in every instance I have found an inner opening to it. This affords a very reasonable explanation of the formation of these abscesses; it is almost impossible to understand, on any other ground, why suppuration should take place in the vicinity of the rectum more than in any other part of the body, and why the cellular membrane there should suppurate more than cellular membrane elsewhere. Moreover, the pus contained in

an abscess near the rectum scarcely ever presents the appearance of laudable pus,—it is always dirty-coloured and offensive to the smell,—sometimes highly offensive, and occasionally you find feculent matter in it quite distinct. There is no reason why an abscess simply formed in the cellular membrane should smell of sulphuretted hydrogen; but there is a good reason why it should do so if it be connected with the rectum.

This being the case, it is easy to understand why these abscesses do not heal. The least quantity of mucus even from the gut, or of feculent matter, issuing into the cavity of the abscess, is sufficient to occasion irritation and prevent it healing, and I have more than once, in the living person, been able to trace the progress of the formation of one of these abscesses. For example, I was sent for to see a lady who complained of some irritation about the rectum, and on examining it I found an ulcer at the posterior part. I ordered her to take Ward's paste,—confec. piperis nigrum, or cubebs pepper, I forget which. A month afterwards she again sent for me, and I found that there was an abscess. I opened it, and from the outer opening the probe passed into the gut through the ulcer which had been the original cause of the disease. The original opening of the abscess is generally very small indeed, but occasionally it is large, and when the ulceration has proceeded to some extent, large enough to admit the end of the little finger. The inner orifice is, I believe, always situated immediately above the sphincter muscle, just the part where the fæces are liable to be stopped, and where an ulcer is most likely to extend through both the tunics.

I believe that the most common cause of abscess of this kind is, the lodgment of hard fæces in the bowels; by the straining that takes place to expel them the mucous membrane gets torn or abraded at one part, and then the passage of the fæces causes ulceration. Some time afterwards straining again occurs, and then the muscular tunic gives way, and the fæces escape into the cellular texture. Foreign bodies, however, in the rectum, sometimes cause abscesses. I mentioned two cases in the last lecture, but I shall mention them again. I mentioned them before because they particularly appertained to the subject we were then discussing. I was called in by a gentleman who complained of great irritation about the rectum. I thought that he laboured under internal piles, but the next day he complained still more, and on examination of the rectum I found a hard substance sticking in the membrane. It was a piece of apple-core which he had swallowed the day before, and if it had not been extracted it would have occasioned ulceration, some of the fæces would have been pressed through the opening, and in all probability the apple-core would have been found in the cavity of the abscess. I was sent for to see another gentleman who was exceedingly ill with a large abscess in front of the anus. He had a brown or rather a black tongue, and bad typhoid symptoms. I opened the abscess freely, let out a quantity of putrid offensive matter, and, on introducing my finger into the abscess, I found a long fish-bone sticking across, with one end in the gut and the other in the abscess. He had swallowed the bone, it had stuck in the bowel, and a little of the fæces escaping by the side made a putrid abscess. Patients with disease of the liver, disease of the lungs, and in certain states of ill-health, are specially liable to abscess and fistula of the rectum. The reason is this: persons thus affected are peculiarly liable to ulcer of the mucous membrane; one of the mucous glandules is attacked, and being very thin it gives way under the straining that takes place to expel the fæces, and fæces escape through the opening.

The first formation of an abscess about the rectum is not in general attended with very urgent symptoms. The patient has a sense of bearing down, a fulness and weight; he thinks that he has got piles, he puts his hand by the side of the anus, and finds a little hardness. After a time it increases, the parts become tender; there is pain when he passes his evacuations; perhaps some difficulty in passing them; by and by the pain becomes still greater, the skin inflames, the abscess, if left to itself, bursts, and a quantity of matter is discharged, which matter is almost invariably offensive, dark-coloured, and putrid. The disease sometimes forms so insidiously that the patient is not cognizant of it till the abscess has burst. Twenty years ago a physician in large practice in London felt very ill, languid, listless, unfit for business; and in the middle of the day, in consequence of headach and an incapability of exertion, wanted to go home and lie down for an hour before he could finish seeing his patients. One afternoon, intending to walk home, he had sent away his carriage. He found something give way, burst into his small-clothes, and on his return he found that it was a putrid abscess—a fistula. He went through an operation for it, and got well.

While these abscesses are forming there is sometimes little or no constitutional disturbance; but in other instances there is a great deal of it, and I believe that it depends chiefly on the quality of the pus, and that, again, on the size of the opening. If the opening be pretty large, and a considerable quantity of feculent matter escapes, the pus is then of a very putrid quality, and the more putrid its character the more offensive it is to the smell, and the more poisonous it is to the patient's system; for as it is more offensive to the smell so it is more loaded

with sulphuretted hydrogen. Such a collection of putrid matter sometimes produces very urgent symptoms. I was sent for to see an elderly gentleman in the neighbourhood of London with the late Dr. Blickham. I will not say that on my arrival the patient was in *articulo mortis*, but he looked as if he had not long to live—I should say hardly twenty-four hours. On inquiring into the history of the case I ascertained that he had a fistula by the side of the rectum. He had suffered under it for many years; for being afraid of an operation he had let it go on. The external orifice occasionally closed for a time, but in a few days it opened again, and gave exit to the matter. Two or three months, however, prior to the time of which I am speaking, the outer orifice had closed, and there had been no discharge, and at first no inconvenience had been felt. By and by there was a sense of pressure, a bearing down of the rectum, and the patient became very much out of health. At last typhoid symptoms supervened, and he appeared, as I have said, almost dying. I examined the parts externally, and saw that the orifice of the fistula had cicatrised. I then introduced my finger into the gut above the sphincter muscle, and I could feel an immense tumour on one side, which was evidently a large collection of matter. With the forefinger of one hand in the rectum, with the other I ran in a lancet up to the point where the matter was collected. Not only the shoulders but the whole blade of the lancet was buried before matter escaped, and then there was a little putrid discharge. With a probe-pointed bistoury I dilated the opening, and there came away a pint of such putrid matter that the whole house was poisoned by it; it could be smelt almost as bad as if a nightman had emptied his cart into it. The patient was better directly; though the incision was large there was no bleeding, and he recovered without a bad symptom. This circumstance took place many years ago, and he died lately of another complaint.

I have stated that the inner orifice of the abscess is always just above the sphincter muscle, and it may be that the abscess extends no higher than this. But in a great number of cases it does extend higher up—sometimes one inch, sometimes two; nay, I have sometimes found a probe pass four or five inches up the pelvis into a large cavity beside the rectum. These are cases of some interest, respecting which I shall have to speak to you again presently.

The external orifice of the abscess is generally in the skin, a little distance from the anus. Sometimes it seems to pass through the substance of the sphincter muscle, and on other occasions it opens externally to it. The abscess may burrow, and may be two or three inches away from the anus.

In some cases there is no external opening at all, and that may happen in two ways. I saw a gentleman who had an ulcer at the posterior part of the rectum as broad as a fourpenny piece. Some time afterwards I saw him again, and there was then a considerable discharge from the rectum, but no external opening. I introduced my finger into the rectum, and found that this broad ulcer had made a large cavity, in which matter was lodged, by the rectum. The sinus was so large that the matter had found its way out by the gut, and therefore did not burrow so as to make an external opening. But in other cases there is no external, while there are two internal openings, and they are found in the following manner:—There is a small opening through which the pus and fæces were originally infiltrated into the cellular membrane, and then the matter having collected near the gut, bursts into it, and makes a free opening in the neighbourhood of the first lesion. On examining the patient you find a discharge of pus from the inside of the rectum, and on introducing the finger you find distinctly the opening through which the abscess has burst into the rectum. This is what is commonly called blind fistula. The discharge in these cases is seldom quite constant; for the opening made by the bursting of the abscess into the rectum is not so large but what it sometimes contracts, and there not being a free discharge the matter collects, and you may feel it through the skin near the anus. This is important with regard to the treatment, as I shall explain hereafter. At other times the orifice allows the matter to escape by the rectum, and then the external tumour disappears.

In some cases there is a simple abscess and a simple sinus; but in other instances the disease is very complicated. The matter does not easily get to the surface, and it burrows in different directions; there is a sinus in this direction, and a sinus in that; sometimes it extends even to the middle of the nates, and there may be a sinus on both sides of the rectum. In these cases, where there are several sinuses, and where the disease is rendered complicated from the burrowing of the matter, it sometimes happens that there are two internal openings; but in general, however complicated the case may be, there is only one internal opening, and that communicates directly with one sinus, and indirectly with another. It is of great consequence to bear this in mind as connected with the surgical treatment. Where there are several sinuses, burrowing in different directions, the patient always experiences some degree of inconvenience. The matter lodges in one place, not in another, but wherever it lodges it occasions pain, there is an attack of shivering, and then the matter escapes. It then lodges in another place there is another attack of shivering, and in these complicated cases the patient is continually suffering local pain and tender-

ness, and these are combined with constitutional disturbance.

I now come to consider the treatment of these cases. Why is it that the abscess does not heal? It may, as I supposed formerly, partly arise from the unfavourable locality for healing, in consequence of the muscular fibres of the parts being always in motion. The levator muscle and the sphincter ani are constantly drawing the parts asunder, so that they are not allowed to contract, but that is not a sufficient explanation. There is an internal opening to the abscess, and now and then a little bit of fæces or mucus will become infiltrated, and get into the cavity. That which produced the abscess originally is going on still. If you could get the inner orifice to close the patient would soon recover. This does sometimes take place. I saw a patient who had an abscess by the side of the rectum, and to whom I recommended an operation, but for some reason or other he wished to put it off. He went about for a considerable time with this abscess, and when I saw him again the abscess was closed, and had been closed so long, and on a careful examination the parts seemed so sound, that I had no doubt that the inner orifice had healed spontaneously; the escape of feculent matter was thereby prevented, and all the parts granulated and contracted like an abscess elsewhere. The medicine which we now call confec. piperis nigri was originally a quack medicine known by the name of Ward's paste. It is composed chiefly of black pepper and elecampane, and it had the reputation of curing fistula. I believe that it sometimes did so. It is very useful in the case of piles, and where there is an ulcer of the rectum unconnected with fistula. The black pepper mixes with the fæces, it passes down the canal, and becomes a stimulant to the mucous membrane. In this point of view it is useful to persons that suffer from disease of the mucous membrane after dysentery, or who have disease of the rectum. As it will cure piles and an ulcer of the rectum, so no doubt it will sometimes cure fistula. If the little ulcerated opening can be made to contract and cicatrise there is no reason why the external abscess should not heal. But you cannot depend on such a mode of treatment as this; it may or it may not happen to cure the patient, and for one instance in which it effects a cure it fails a great number of times. The disease, however, may generally be cured by a very simple operation, and in speaking of the operation we will take the simplest case first. We will assume that there is a fistula just by the side of the sphincter muscle and only one sinus. The first thing to be done is to find the inner opening. I do not say that you will always succeed in finding it—certainly not the first time, but you will rarely fail if you look for it in the right place. Formerly, I often failed, and for this reason,—I did not know where to look for it. I used to think that it was to be found in the upper part of the sinus, but it is never found there if the sinus runs high up. You must search for it immediately above the sphincter muscle. Another circumstance that makes it difficult to find is this:—The common probe being quite round turns round in the hand; you want a probe of a much broader kind, so that the least motion of the hand turns the point another way. For this operation I use the probe I now show you, made by Philp and Wicker, in St. James's-street. First, it has a flat handle, and that gives you a perfect command of the instrument; secondly, at the extremity it is like a common probe; but you must have probes of different sizes. There is a groove, so that it is both a probe and a director at the same time, and being made of silver it is perfectly pliable.

Now, to find the inner opening place the patient over a table to the light, with an assistant to hold the legs. You introduce the fourth finger of the right hand into the rectum, remembering that the opening is close to the sphincter muscle. You will feel with the finger some little irregularity, and that is where the opening is probably situated. You are then to introduce this probe into the external opening with the assistance of the finger in the rectum, using no force, and by a careful manipulation feeling first in one direction, and then in another, at last it will almost alone pass through into the rectum. It must be done gently, and a little practice will enable you to find the inner opening. You ascertain when it has passed through the opening by its coming in contact with the finger. If you do not find the opening the first day put off the operation to another day. Occasionally I have tried two or three times before I could discover the opening, but generally, if you have probes of different sizes, it is easily found. Sometimes the opening is very small, and therefore requires a small probe. When you have found the inner opening, and the probe is in contact with your finger, you bend the end, and bring it out at the anus. Thus, the part towards the handle is seen projecting from the outer opening, and the other part from the anus, while the parts which are to be divided lie upon the groove of the director. I generally divide the fistula with a pair of curved knife-edged scissors, for they cut better than a bistoury. A bistoury tears, and you may cut you own finger if you use the sharpedge. Introduce the scissors along the groove of the director, and divide the parts that lie between the inner and the outer orifice. There is scarcely anything to be divided—not above an inch or an inch and quarter, but you divide the greater part of the sphincter muscle.

Having performed this operation, all you have to do is, to prevent the cut edges growing together. You have made it into

a sore, some of the fæces go into the sore, but they do not lie and lodge there, and there is nothing to prevent this fistula which is now made into an open sore granulating and healing. All you have to do is to dress the parts very lightly between the cut edges to prevent them growing together, and that must be continued till the cut edges are skinned over. You may then leave the parts alone, and the healing process will go on.

But suppose that the fistula extends high up by the side of the rectum, above the opening, and this fistula is burrowing, what is then to be done? I used to imagine formerly that it was necessary to lay open the whole sinus into the rectum, but it is a frightful operation to lay open so long a sinus. You do not know what vessels you divide. There is seldom much bleeding in dividing the parts between the inner and the outer opening, but if there be much the pressure of the finger and a bit of lint stops it directly. I remember a case where I divided a fistula some way up by the side of the gut, and the whole canal was filled with blood. It is true the bleeding stopped, and the patient got well, but still he might have died from hæmorrhage. The bleeding goes on insidiously; you do not know how to stop it; it is internal, you cannot take up the vessels, and you cannot make pressure in any efficient manner. But I am now satisfied, and have been for a long time, that the division of a fistula which extends above the inner orifice is quite an unnecessary proceeding. Upwards of twenty years ago, when I was first getting into practice, I had a patient with a fistula, which I divided, or, at least, thought I had done it. But one day, when examining it with a probe, I found a sinus running up by the side of the gut for several inches. It seemed as if one side of the rectum was completely dissected from the neighbouring parts, but there was a good opening at the lower part where I had divided the fistula. Not knowing what to do with the case I called in the late Mr. Cline, and observed to him that if I divided it the whole length the patient might die from loss of blood. He said, "You are quite right, but more than that I do not think it is necessary, I would leave it alone." There was a free opening below; the fæces could not escape so easily now, and get into the cavity above. I adopted his advice, and the patient got well without any trouble. I have since seen other cases of the same kind. Where there has been a large sinus, connected with a fistula, I have laid open the parts between the inner and the outer orifice,—done nothing more,—and the patient has got well. If a very long sinus, and a very large cavity, heal up without being laid open, *à fortiori*, if there be a small sinus, and a small cavity, that will heal up to.

In the next lecture I shall call attention to the treatment of more complicated cases.

HOSPITAL FOR THE CURE OF DEFORMITIES.

COURSE OF LECTURES ON THE DEFORMITIES OF THE HUMAN FRAME.

BY

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Lecture XI.

Contracted hand and fingers from disease of palmar fascia. Probability of origin from gastro-intestinal disturbance and rheumatic diathesis. Spasmodic contracture of fingers and hand. Treatment. Non-congenital wry-neck from spastic contraction, paralysis, rheumatism, voluntary retention. Treatment. Utility of section of sterno-cleido-mastoideus.

To-day, Gentlemen, we will devote a few minutes to the treatment of contracted finger. When one or more of the articulations of a finger become contracted from disease of the articulations, often complicated with thecal abscess, the case constitutes false ankylosis of the affected articulation, the treatment of which has been already described. I may here remark, that such deformities of the finger do not admit of restoration, or if the bent condition be removed by tenotomy, the function is not always restored, either in consequence of the tendons having sloughed, or from their having become adherent to the sheath during the primary disease, or from the articular surfaces having received irreparable injury. But in *contracture* of the fingers the abnormal flexion has resulted from causes which have operated indirectly on the articulations. These causes may be superficial inflammation of the hand and fingers, chronic disease or injury of the palmar fascia, superficial wounds of the finger, injury to the flexor tendons, rheumatic inflammation, and, lastly, disease in the nervous system, in which case the contracted finger ranks as a spasmodic or paralytic contracture.

One of the most interesting and common forms of contracted finger is that resulting from disease of the palmar fascia, as at fig. 21. In this affection the middle, ring, and little fingers are principally liable to contracture. I do not remember to have witnessed contracture of the forefinger from this cause. This probably depends on the anatomical distribution of the fascia. The contracture commences without obvious cause, the patient's attention being attracted to a stiffness in the palm, and an incapability of completely straightening one or more

of the fingers. The contraction and stiffness increase until the tip of the ring or middle finger is drawn completely into the palm, greatly interfering with the functions of the hand. The contracted condition of the finger is constant, not varying in extent at different periods, as some contractures from other causes. Its progress is slow, several years elapsing before the deformity attains the highest grade.

An examination of the finger shows that the deformity is maintained by an indurated, inelastic, hypertrophied condition of the palmar fascia. The individual retains the power of flexing and extending the finger, within the range permitted by the contracted fascia. A tense cord is perceived extending along the palm, towards the most contracted finger, in part arising from shortening of one of the flexor tendons thrown into relief by its contraction. The habitual state of the hand in the individual from whom this model (fig. 21) was taken, was one of

Fig. 21.

Contracture of Fingers from Disease of Palmar Fascia.

greater contraction; the model represents the appearance when the patient endeavoured, by muscular action, to extend the fingers. The pathology of this affection is interesting. From the inability of patients to assign any cause for the contraction, I have been induced to search for it in some abnormal condition of the general system. I have witnessed instances of the contracture in persons of different trades and professions, coachmen, bakers, artisans, clerks, and individuals of richer classes, following no manual occupation. It usually affects both hands, though in different degrees, and sometimes the soles of the feet present a similar affection of the plantar fascia. Every patient had suffered from some form of dyspepsia, from rheumatism and lithic acid urinary deposits, and the majority admitted that at some period of their lives they had partaken freely of wine, or other fermented liquors. Hence a rational pathology leads to the conclusion that this induration and thickening of the fascia is one of the forms in which a rheumatic or arthritic diathesis manifests itself. It is apparent that the affection of the fascia is primary, and that the contraction of the flexor tendons is secondary, consisting of structural shortening from long continuance of the deformity, independent of any lesion of innervation of the muscles of the forearm.

Treatment.—Subcutaneous section of one or more of the bands of contracted fascia, extending towards the fingers, and of the subjacent flexor tendons. These operations should be done in the part of the palm where the tension is most evident, and with the view of preventing adhesion of the tendon to other divided structures, as in a cicatrix; and I have preferred severing the tendon apart from the fascia. I have witnessed no case in which the puncture has not healed by adhesion within forty-eight hours. After adhesion the contracted fingers should be gradually straightened by being bandaged on a splint. It is not desirable to effect this part of the treatment too rapidly, as great pain will be produced by the attempt. Three weeks or a month usually suffice for removal of the resistance presented by the various textures of the palm that have been long contracted (for it should be remarked that the abnormal condition of the fascia renders the palm unnaturally concave). The use of the fingers is entirely restored, and at the expiration of a few months, the induration and thickening of fascia being slowly removed by absorption, no trace of the deformity will remain, as in this model (fig. 22), which re

Fig. 22.

Cure of Contracture of Fingers from Disease of Palmar Fascia.

presents fig. 21 after the cure. In this case section of the band of fascia, and of the tendon proceeding to the middle finger, was the only operation requisite, the consequen

relaxation of the fascia and loosening of the palm permitted the complete straightening of the remaining fingers.

Similar contracture of a finger from a wound of the palm, or of the finger itself, although the wound may have united by adhesion immediately after the injury, is not unfrequent. The treatment should be conducted on the principles already laid down.

The *spasmodic* contractures of the fingers are more complicated in their pathology and more difficult and uncertain in their treatment. This state is often associated with spastic affection of the wrist and arm, as in this model (fig. 15, p. 352), with spastic affection of the toes, or of the face or neck. You will experience no difficulty in distinguishing it from paralytic contracture, which it so greatly resembles in appearance, by remarking that the patient possesses power of volition in the extensor muscles, but that these muscles are overpowered by the spasmodic condition of the flexors. The contraction of the flexors is often more convulsive or clonic than spastic.

Here are models illustrative of the different grades. In the majority you perceive the thumb is firmly drawn into the palm. I cannot speak favourably of tenotomy when practised where the tendons are inclosed in the thecæ, but when effected in the palm it has proved more successful. In some instances, where the affection was combined with abnormal flexion of the wrist, I have divided the flexor sublimis digitorum, the flexor longus pollicis, flexor radialis and ulnaris, above the wrist, with much advantage. The operation has been less directly beneficial in restoring the use of the member than as serving as a starting point for improvement. Thus, the patient after the operation, is sensible of the removal of a powerful obstacle to the action of the extensors, and is enabled to use them more effectually in controlling the remaining active flexors (for in these cases the flexors generally are involved). Persevering education of the muscles becomes necessary for the obtainment of an useful member. In some instances I have succeeded so far in restoration that the individual has been enabled to use the hand in coarse manipulations, and in one case to write tolerably well.

Another deformity, constituting a *contracture*, is the non-congenital wry-neck, or caput obstipum.

This affection is characterised by an inclination of the head to one side, and a peculiar rotation, as in this drawing (fig. 23), through which the ear is approximated to the upper part of the sternum. In some cases the deformity is so considerable that the head almost rests on the shoulder.

You will, from my classifying this affection with contractures, at once conjecture that it may be produced by a variety of causes.

Fig. 23.

Wry-neck.

Any circumstance, in short, which tends to disturb the equilibrium of the muscles which are attached to the thorax and to the head may induce the deformity at any period of life. Spasmodic contraction, paralysis, rheumatism, voluntary inclination of the head to one side during painful affections of lymphatic glands, during vesication from cantharides, other irritating applications, or the cicatrix of a burn. Many of these causes act generally on the muscles of one side, and the deformity may, in the first instance, consist only of a simple leaning of the head to that side, but in consequence of the bulk and power of the sterno-cleido-mastoideus, and the freedom of its course being unattacked except at its extremities, this muscle, by reason of its peculiar physiological action, promptly usurps a predominant influence, even when not specially affected, and converts a simple yielding of the head into a complicated deformity. The diagram before you exhibits a slight case; when more severe the opposite shoulder is greatly depressed, and a curvature of the cervical vertebræ induced. An attentive examination will enable you to discover the structures which principally impede the restoration of the position of the head. The most prominent, and in many cases the sole resistance, is afforded by the sterno-cleido-mastoideus. Sometimes the tension of the trapezius evidently modifies the deformity, and often the more deeply situated muscles appear involved, although the belief in this universal muscular affection is derived rather from observation of the effects of treatment than from examinations previously to it. In the majority of instances the sternal portion of the sterno-cleido-mastoideus alone appears tense; sometimes the clavicular portion is especially contracted, in which case the clavicle becomes deformed and the central part is drawn upwards. I was consulted respecting an interesting case of this variety by Mr. Buchanan, of Ratcliff.

The patient is unable to rectify the position of the head, and seldom will you succeed in accomplishing it by forcible manipulation.

I shall now describe to you some peculiarities of the different non-congenital forms of wry-neck, and then proceed to the consideration of their treatment. I must refer you to the lecture on contracture in general for many observations which apply to wry-neck. The spastic form is the most common, and occurs suddenly or gradually, from disease within the nervous centres, manifested, for the greater part, through the instrumentality of the spinal accessory nerve, of which the filaments are distributed to the sterno-mastoideus and trapezius muscles. Sometimes the spasmodic contraction is transient, or much augmented under emotion; at other times the tension of the muscle is constant, and structural shortening usually exists to a greater or less extent. The paralytic form is not unfrequent; in these cases the deformity is maintained by contraction of those muscles which are deprived of antagonists. Wry-neck consequent upon voluntary retention of the head on one side occurs, almost exclusively, in young persons. In these the deformity is rapidly produced, apparently in consequence of the continued growth of the vertebral column and of the structures on the side of the neck, towards which the head does not incline, whilst the growth of parts on the inclined side being retarded, a disproportion between the muscles on the two sides is produced. The deformity resulting from burns is usually of a complicated nature, as the tension of cicatrices, resulting from loss of substance, is added to the muscular shortening induced by long-continued rest in a certain position.

A deformity similar to wry-neck is sometimes produced by disease of the cervical vertebræ. You will, therefore, be careful not to confound these different affections.

Treatment. — Non-congenital wry-neck requires treatment conducted on principles similar to that of other contractures. In the spasmodic and paralytic forms, if the deformity have not existed many years, you may employ the general remedies applicable to disease of nervous centres, already enumerated, seldom with complete success. Should the deformity be recent, and have proceeded from rheumatism, disease of cervical glands, or any local cause exterior to the central organs of the nervous system, the removal of the local affection, by appropriate topical and other remedies, must be effected previously to special treatment of the deformity. If the origin be satisfactorily proved to be voluntary retention of the head on one side, continued vesication or other painful counter-irritation on the opposite side may be resorted to with advantage. I have in two instances witnessed the success of this plan. I need scarcely advert to the necessity of a correct diagnosis in this, as indeed in all other cases of disease or deformity. It may occasionally be very difficult to form a correct opinion. I have recently witnessed an illustration of this fact, in a case in which Sir Philip Crampton, Mr. Adams, of Dublin, and I, were consulted. In all cases, except the spasmodic and paralytic, you will derive benefit from manipulations of the head and neck, exercise of the weakened muscles, and the wearing of a stiff leather or pasteboard collar, properly padded, and proportionately higher on the depressed side of the neck. In every case in which structural shortening of the sterno-cleido-mastoideus exists, tenotomy of one or both of the origins of the muscle may be effected. Sometimes section of the sternal portion is sufficient, but instances occur in which the cure will not ultimately be effected without section of the clavicular portion. You will remark in these cases that after the resistance of the sternal portion is removed the clavicular becomes daily more prominent, and partially retains the deformity. In severe spasmodic cases *subcutaneous* tenotomy is especially useful; it does not always effect a complete *cure*, because the disease resides in the central organs of the nervous system, which tenotomy cannot effect. But the operation, succeeded by appropriate treatment, lessens the energy and power of the muscular contraction, relieves the distressing neuralgic and other pains accompanying this form of deformity and renders more effectual the use of the collar that may subsequently be required.

It serves also in many instances to attain the very important object of enabling the muscles that have been long overpowered to recover a large portion of their lost vigour. I have witnessed the necessity of repeating the operation in a severe case of this nature. Stromyer relates a similar case. I believe the relief afforded by tenotomy, in very active spastic cases of wry-neck, to be so considerable, whilst the pain and inconveniences of the section are so trivial, that a person thus affected may be readily willing to consent to its repetition.

Tenotomy does not, *à priori*, appear indicated in a paralytic case, yet if structural shortening exist the section may properly be resorted to, simply as a means of restoring the position of the head, and facilitating its retention by an appropriate cravat.

Various ingenious contrivances have been proposed for maintaining the head in a proper position, either after tenotomy, or when the operation is unnecessary, the most notable of these is that of Joerg. It does not completely answer its purpose. The cravat is more commonly employed. The most simple and effectual consists in encasing the head with a common bandage, and in securing it by means of appropriate slips to the shoulder, or any part of the dress of the individuals.

Deformity of the head and neck from burns are rarely remediable,—tenotomy may occasionally be useful; but the plastic ope-

rations are more frequently serviceable. I cannot speak of these from my own experience, and therefore shall not detain you longer on this subject. At the next lecture we shall consider the deformities resulting from rachitis, or rickets.

[We are requested by Dr. Little to state that he is indebted to Mr. Goldsmith, surgeon, Islington, for the drawing of the skeleton of a Chinese lady's foot, in our number of the 23rd ult. The dissection was made by the surgeon of H.M.S. Algerine.]

AN

ANALYTICAL INQUIRY

INTO THE NATURE OF

THE PROCESSES OF DIGESTION AND NUTRITION.

WITH PRACTICAL DEDUCTIONS.

By George Ross, Esq., Kennington.

(Continued from page 510.)

The Digestion of Sugar.

With a view to ascertain the nature of the changes that take place during the digestion of sugar, and their correspondence with those induced by artificial digestion, I fed a rabbit upon this substance, and examined the contents of the stomach and small intestines within an hour after the meal. The animal had not been fed since the previous evening, a period of fifteen hours having intervened; yet, on examination, the contents of the previous meal were found in the stomach, whilst the colon, also, was distended with a pultaceous mass in a more advanced stage of digestion. The contents of the stomach did not evidence any lactic acid, nor was there any coagulability, either by heat or nitric acid.

The small intestines contained a large quantity of fluid resembling the white of egg, mixed with portions of mucus and biliary colouring matter. This fluid contained sugar and a copious amount of lactic acid; it was not coagulable by heat, but largely so by nitric acid and by muriatic acid; the precipitate, in the latter case, being rendered soluble by excess of acid, which was also facilitated by heat. It was likewise precipitated by the acetate of lead.

It was apparent that the sugar, in this instance, had been converted into lactic acid, precisely in the same mode that sugar is thus changed by the action of animal substances out of the body. The substance incoagulable by heat, precipitated by muriatic acid, and rendered soluble by excess of acid and by heat, partook rather of the character of casein than of albumen.

A similar principle has been called, by Dr. Prout, incipient albumen, on the supposition that it is albumen in the nascent state; but this term does not appear to me to express with sufficient precision the characters of the above substance, while it indicates, at the same time, more than we are warranted to assume. We are not certain that this principle is albumen in its transition, or rather formative state; but I think that there is sufficient evidence for a conclusion that this principle is albumen in a state of decomposition induced by the *reducing* powers of the digestive organs, which inculcates a very different idea from the term incipient albumen. I shall hereafter endeavour to indicate with more accuracy the propriety of this term, or of the idea it implies in connection with other principles.

It is difficult to determine how much of the contents of the intestines depends upon the secretions poured into them, and how much upon the food. The pancreatic juice, for example, contains casein, and since this fluid is constantly present in the alimentary canal during digestion, we might expect to find therein a proportion of this principle. The same fluid abounds, moreover, in albumen, yet I could not detect albumen, strictly so called, in the intestine. It is, I apprehend, necessary that the albumen poured into the alimentary canal by the secreting glands should undergo the operations of the digestive process, like albumen derived from the food, before it can be re-absorbed for the purposes of nutrition. Hence, the albumen produced by secretion may, in the small intestines, be converted into a principle analogous to casein. I presume that in the foregoing case the coagulable principle was chiefly derived from the secretions.

Digestion of Starch and Sugar.

I fed another rabbit, at the same time, on a paste made of equal parts of arrow-root (*pure starch*) and sugar; but, on account of the extreme difficulty of inducing the animal to swallow this kind of food, I was able to pour only three teaspoonfuls into its stomach. On examination, an hour and a half afterwards, this organ was observed to be filled with the same contents as in the former case, and the fluid presented similar characters. The duodenum and small intestines, however, contained so small a quantity of fluid that I could procure scarcely a drachm for observation. Sugar was not manifest; there was a small proportion of lactic acid, and the coagulable part presented the same reactions as in the former case. It is probable that, in this instance, the small quantity of fluid found in the intestines was partly attributable to the limited quantity of the food admitted into the stomach, and partly to the process of absorption.

Sugar, during digestion, apparently undergoes peculiar changes, of which the formation of lactic acid is the result. This acid is absorbed by the radicals of the portal system as soon as it is formed, and, consequently, any accumulation of this acid in the intes-

tines is prevented. Further experimental research, will, I conceive, be found in accordance with this conclusion, which derives stronger corroboration from circumstances developed during the process of artificial digestion.

The Artificial Digestion of Albumen by Schwann.

For the better comprehension of the experiments that will follow, I will subjoin a brief account of the conditions of artificial digestion as conducted by Schwann, and afterwards give the chemical characters of casein. This chemist found that when albumen was macerated in a test-tube with an acidulated solution of the digestive principle, it became transparent, but cheesy in the centre. After twenty-four hours it was quite dissolved; the albumen was now no longer coagulable; the solution contained osmazome and salivary matter, and a third substance, similar to albumen, insoluble in water and alcohol, soluble in dilute muriatic and acetic acids, precipitated by carbonate of soda, not precipitated by boiling heat, acetate of lead, nor alcohol, but thrown down largely by nitric acid and oxymuriate of mercury. Now, it is manifest that, in the properties of the third substance of artificial digestion, there is a disposition in the albumen to assume the characters of casein, this principle being chiefly distinguished, as we shall presently see, by its precipitation in small quantities of dilute acid and resolubility in excess of acid, and by its incoagulability by boiling heat. It will be further observed that the coagulable portion of the fluid found by me in the small intestines of the rabbits corresponded with this substance in these essential points.

Chemical Characters of Casein.

Casein is chiefly remarkable for its precipitation by pepsin, and for its being rendered, at the same time, insoluble in water. Pure casein is, otherwise, soluble both in warm and cold water, and is not affected by the boiling temperature. Acids, in *small* quantity, precipitate the casein; in *excess, redissolve it*. Pure casein is precipitated by alcohol, corrosive sublimate, alum, and acetate of lead; if these reagents are removed by washing, the precipitate, in each case, is resoluble in water. The peculiarity of the action of these reagents on casein is, that they cannot (with the exception of pepsin) render this principle permanently insoluble. Hence, casein is more closely allied to protein than any other azotised principle.

Preliminaries of Artificial Digestion.

In order to observe and distinguish the different effects produced on albumen and casein by artificial digestion, I precipitated casein by rennet, from milk, and having formed cheese, submitted it to the action of various reagents. When cheese is thus formed the casein is precipitated in combination with butter and albumen; and the azotised substances having undergone the action of pepsin, we obtain the necessary conditions for conducting artificial digestion.

Artificial Digestion of Albumen.

When casein, precipitated by pepsin, had been macerated in dilute acetic acid twenty-four hours, there was no effect in the solution by heat or alcohol; carbonate of soda caused a pearliness; bichloride of mercury, a more copious precipitate; acetate of zinc caused a pearliness; ether caused a pearliness, clarified by heat.

When cheese had been macerated in dilute muriatic acid twenty-four hours, there was no effect produced in the solution by heat or alcohol; carbonate of soda caused a *copious* precipitate; bichloride of mercury a pearliness; a few drops of acid being added, these precipitates were immediately *redissolved*; acetate of zinc threw down a pearliness, not clarified by excess of hydrochloric acid. Ether, no effect.

These effects clearly depended upon the presence of albumen, since they closely corresponded with those reactions observed to take place by Schwann on submitting the third substance of the artificial digestion of albumen to the same tests. Moreover, when the cheese was macerated a second time in acid no effect was produced by these reagents; thus proving that the albumen soluble in acid had been removed, while the insoluble cheese had been left behind. Osmazome appears, also, to have been present. I observed another important fact with respect to this reduced albumen, namely, its precipitation in a small quantity of acid and its resolubility in excess of acid, indicating, thereby, an approximation in this substance to the characteristic properties of casein. Hence we cannot but conclude that it is the office of the digestive process to form casein, or a similar substance, from albumen. The pearliness caused by ether in the acetous solution shows that this reduced albumen corresponds rather with the *albumen of the egg* than of serum, since the same effect takes place in the former case, but not in the latter.

Caseum subjected to the Action of an Alkali.

After the second maceration of cheese in dilute acetic acid, and a subsequent careful ablution, by which all the albumen was removed, it was submitted to the action of solution of potash. On testing the solution it was precipitated by dilute acid; only partially redissolved by strong acid; it was precipitated by corrosive sublimate, not again redissolved by washing; alcohol did not affect it; heat, at first, rendered it more transparent, but continued boiling precipi-

tated it in clots. When the solution in liquor potassæ was neutralised by nitric acid, and the solution afterwards filtered, the precipitate thrown down by muriatic acid was *entirely redissolved* in more muriatic acid; when carbonate of soda was added to this, a slight haziness was observed by transmitted light, when the action between the acid and alkali took place; but there was no decided precipitation. These experiments were repeated with the same results.

After the maceration of caseum in dilute muriatic acid it was dissolved by liquor potassæ. The solution was not affected by heat, but largely precipitated by dilute acetic acid, which precipitate afterwards formed a dense scum. Hydrochloric acid cast down a similar, but more abundant, coherent scum. This scum was easily soluble in warm water, and remained dissolved on cooling; it was largely precipitated from this solution by dilute hydrochloric acid, and redissolved by this acid in considerable excess. After the scum was well washed it was subjected to rennet, and pearliness was produced. Thus I procured the casein in its primitive state. This substance was distinguishable from protein by its solubility in water. There was a considerable evolution of gas during the action of the hydrochloric acid.

Rationale of the Formation of Albumen.

The information obtained from these reactions is curious, and seems to show that if casein, after precipitation by pepsin, enter into combination with an alkali, it is converted into a substance nearly resembling albumen. This fact may throw some light on the production of albumen, and explains the phenomena of the constant occurrence of uncombined alkali in albuminous fluids. We observed that whilst the caseum was in combination with potash its precipitates were comparatively *insoluble*.

On the contrary, when the potash was neutralised, the precipitate by acid was entirely soluble in excess of acid, reassuming the characters of casein; or, more properly, that modified form of casein which ensues upon the artificial digestion of albumen, since we find that the fluid was affected by carbonate of soda, which never happens, according to my observation, after the maceration of caseum that has not been changed by an alkali, but is distinctive of the reduction of albumen. Hence, then, we have reason to believe that caseum, acted upon by an alkali, is really converted into an incipient albumen.

I was further anxious to ascertain the effects of lactic acid, or sugar, on these azotised substances, and for this purpose instituted a great variety of experiments, of which I will now enumerate the most important.

Effects of Sugar in Artificial Digestion.

I submitted caseum to the action of a solution of white refined sugar for *twenty-four hours*. At the expiration of this period the filtered solution contained lactic acid; it was not affected by heat, nor by hydrochloric acid, but nitric acid rendered it cloudy; it was copiously precipitated by the acetate of lead, which precipitate was not redissolved by heat, but immediately *redissolved* by dilute hydrochloric acid; it was also precipitated by bichloride of mercury, which precipitate was neither redissolved by heat nor by hydrochloric acid: in neither of these cases was the precipitate redissolved by washing. The solution was not affected by carbonate of soda.

In another portion, thus treated for a longer period, heat caused a turbidity, clarified by hydrochloric acid. *Ether rendered it cloudy*. Acetate of lead caused a precipitate. Hydrochloric acid caused a precipitate, redissolved by potash. Liquor potassæ precipitated it when the alkali was added in large quantity—a peculiarity in albumen observed by Muller.

This substance was clearly albumen modified by pepsin and lactic acid; it agreed with the properties of albumen in being precipitated by nitric acid, and forming a wholly insoluble precipitate with bichloride of mercury, and it resembled casein in being precipitated by acetate of lead; yet this precipitate was not rendered soluble by washing, thus betraying the more fixed character of albumen. It is evident that the reducing powers of lactic acid on albumen are not so great as those either of acetic or hydrochloric acid, which are the two acids proper to digestion.

The cheese that had been thus treated was thoroughly washed and allowed to stand in pure distilled water for various periods, reaching from one to three days. At the end of these periods there was *no odour of putrefactive decomposition*, and there was no precipitate from solution either by heat, or corrosive sublimate.

The caseum that had been thus macerated in water was again treated with solution of sugar for twenty-four hours. The filtered solution was rendered slightly pearly by heat, and was clouded by bichloride of mercury; alcohol did not affect it.

Another portion that had stood for thirty-six hours, and which was carefully examined, contained lactic acid in the solution. This solution was not affected by nitric acid nor by hydrochloric acid, and the solution acidulated by hydrochloric acid was not precipitated by carbonate of soda. The solution was copiously precipitated by *heat*, which was *immediately redissolved* by hydrochloric acid. Bichloride of mercury caused a slight milkiness in the solution. There was no odour of decomposition.

Another portion was subjected to sugar for forty-eight hours. The filtered solution contained lactic acid, and was not affected by hydrochloric acid; it was rendered hazy by *heat, unchanged* by the addition of acid; it was clouded by the acetate of lead, and was *more abundantly precipitated* on the the addition of *hydrochloric acid*. Corrosive sublimate caused a cloudiness, which was increased by heat and not redissolved by acid. There was no odour of decomposition.

The nature of the reactions varied in the foregoing experiments according to the length of time employed in the maceration. In each instance, however, we observed one uniform peculiarity, namely, the coagulation by heat, which is the nearest approximation I have observed to the ordinary properties of albumen. The nature of this precipitate seemed to vary, however, with the period employed in the maceration; for in the solution that had been only thirty-six hours in contact with sugar, it was immediately redissolved by acid; but in that of the longer period it was unchanged by acid. We are thus led to conclude that there was a gradual change in this substance to albumen. Corrosive sublimate also cast down an insoluble precipitate. It is further remarkable that acids produced no effect on the solution, proving that this substance was soluble in acids like casein. On the whole, it appears that sugar or lactic acid has the property of converting caseum into a substance closely resembling albumen, and that even albumen itself, after it has been subjected to pepsin, seems to acquire some of its original properties under the influence of this acid.

That portion of the caseum, or cheese, that had been submitted to sugar a second time, for thirty-six hours, was now treated with liquor potassæ, and after twenty-four hours was again examined. Butter was on the surface, an insoluble substance beneath it, and a clear solution below. The insoluble substance was *not* dissolved by *heat*, but was redissolved in excess of nitric and hydrochloric acid, and by heat after acidulation with these acids. The solution was precipitated by heat and by the acids, but redissolved in excess of acid and by heat, as in the former case. Liquor potassæ or carbonate of soda added to the acid solution caused slight opacity.

General Remarks.

Ferro-prussiate of potash threw down a precipitate in all the acid solutions, thus reducing the substances to protein; but this salt did not not throw down a precipitate from the simple solution of caseum in sugar until a few drops of hydrochloric acid had been added. The fact of ether producing a precipitate in the solution in acetic and lactic acid shows that the albumen is not the albumen of serum; it also directs attention to the assimilative qualities of white of egg. Heat produced no effect in the solution by hydrochloric acid, but abundantly so in that by lactic acid; consequently, the combination of hydrochloric acid with these bases prevents coagulation. I need scarcely say that it is not the ultimate reduction of azotised matters to protein that I have sought to accomplish, but to ascertain the various characters of the compounds of protein as they pass from one stage to another, and to establish the reality of these transitions.

Necessity of an Acid in Digestion.

It is then palpably manifest that the presence of an alkali renders reduced caseum or albumen insoluble, and, therefore, tends to confer on this substance the original characters of albumen, and these experiments afford a confirmation of the position I deduced in the early part of this inquiry. Further, the characteristic property of casein or albumen that has undergone the digestive process is its solubility in excess of acid. The presence of an acid is, indeed, essential for the solution of this substance and its consequent absorption. There does not appear to be any specific substance into which albumen and casein are alike resolved, but that during digestion they undergo various stages of transition, of which the property common to all is solubility in acid.

Conditions of Putrefactive Decomposition.

I have noted, on several occasions, the non-appearance of the putrefactive decomposition, for this reason, that I have observed when cheese was macerated in water for a few days, decomposition was always betrayed. When cheese was macerated in water for three days there was an odour of decomposition, and modified albumen was precipitated from the solution. If this cheese were thoroughly washed and again macerated for two days, decomposition was apparent; there was *no lactic acid*. Pure caseum macerated in water was not decomposed after this period, as I have shown; whence we may presume that the presence of albumen accelerates decomposition, and it may thus considerably hasten the changes produced during the digestive process. We observe, also, that the presence of lactic acid, in the former cases, was dependent on the mutual action of an azotised substance and sugar.

Deduction.

That sugar holds some influence over the digestive changes induced in azotised substances is apparent, and we cannot question that, up to a certain point, it promotes the conversion of caseous into albuminous matter. It is also clear that the sugar is converted into lactic acid, and that this acid is instrumental in these changes,

Hence we find the reason for the nutritious properties of a mixed diet, since farinaceous food, in the course of its digestion, will assist in the reduction of animal food. We cannot pass over the union of sugar and casein in that aliment the most natural and best suited to the wants of the system of all others, milk. There can be no doubt of the ultimate design of this combination, and the foregoing experiments demonstrate that this design is accomplished by the mutual reaction of these substances, by which digestion is facilitated.

In animals living on azotised food, especially, the presence of an acid is absolutely necessary during the digestive process; and we, consequently, find that free acid is invariably found in those parts of the alimentary canal where digestion is carried on. It is needless to enumerate the particular instances, since I might range over the whole domain of comparative physiology, and because the fact is well established. I must observe, in order that I may not be misunderstood, that caseum is neither soluble in water nor acid until it has been acted upon by an alkali, or by saccharine matter, during its digestive fermentation. Albumen, on the contrary, is immediately acted upon by an acid after subjection to pepsin. Caseum, in short, must be converted into a substance having properties similar to albumen before it can be dissolved by an acid.

The ultimate and more important use of sugar and farina require more particular attention. After these principles have undergone and performed the necessary series of changes in the alimentary canal, they become capable of receiving another series of changes, and of being ultimately converted into other principles. This has been a point that has been hitherto accepted as granted, although the nature of the changes has never been shown. I shall endeavour, in my next paper, to throw some light upon this subject, and although my link may not burn with sufficient brightness to illuminate the whole region of obscurity, yet some other person may be incited to apply his lamp to the flame, and prosecute the inquiry to a yet greater extent. It is an ample field that will richly reward the persevering cultivator.

(To be continued.)

CASE OF

HÆMATEMESIS,

TREATED BY

JOHN EPPS, M.D., London.

MRS. WAITE, aged 23, married only a fortnight. I was requested, on Saturday, July 15, 1843, by the mother of this patient to visit her daughter, whom she represented as in a most dangerous state, and rapidly becoming worse.

I learned that on the previous Monday the patient, to outward appearance, was very well, but in the afternoon of that day was seized with giddiness and faintness, fell, and was found lying on the floor. This was followed by the vomiting of a large quantity of blood, which continued daily more or less, decreasing a little till Friday, when (she having had some *powerful purgative* medicine administered by her medical attendant) it increased, and on the Saturday, the day I was consulted, still increased. Such was the state, indeed, that the surgeon in attendance said if another vomiting took place the result must be fatal.

When I arrived, which was about a quarter past one, p.m., I found the patient lying on the bed with white cheeks, white lips, white blanched fingers, with a swollen transparency about them, exhausted, eyes half closed, pulse rapid, and weak, excessive anxiety of countenance, the tongue blanched, but with a tint indicative of approaching typhus, the teeth encrusted with a sordes, cold sweats often breaking out, and she herself excessively thirsty. Besides these symptoms there was the peculiar *restlessness*, so striking in these cases, causing her constantly, so far as her weakness would allow her, to change her position. Her mother stated further, that the patient experiences continual pain round the waist, this becoming violent before she vomits, the pain being after the vomiting for a short time relieved. Her appetite was gone; her bowels had been very violently acted upon by the medicines administered to her before I saw her; the motions were black and knotty; her water passes regularly; her last monthly period was natural. I satisfied myself that she had had no blow, no extra exertion. I ascertained, also, that she had had pain round the waist for eight or nine weeks before she was married, and also a pain at the heart, the latter continuing after her marriage.

I gave the patient at once three globules of aconite, in a wineglass of water, and ordered the following:—

℞ *Arsenic*, four globules;

Water, four ounces. The fourth part to be taken immediately, and the dose to be repeated every fourth hour.

I left with the patient three globules of veratrum, to be given in case she fainted away.

Sunday morning, July 16. The patient vomited some blood soon after I left her yesterday. She took the arsenicum mixture, slept *in the beginning* of the night, but after waking became restless, and has so continued ever since. She has not passed any water since I saw her yesterday. Her paleness is, of the two, rather worse, and her appearance (to her mother) was worse than it was yesterday; her bowels have *not* acted. She has *not* complained of the pain

round the waist and upper part of the bowels. She fainted yesterday, and the *veratrum* globules were administered. She feels, to use her own words, "heart-sick," retches very much, but brings up nothing; she *wishes to die*. For the continual retching I prescribed the following mixture:—

℞ *Ipecacuanha*, four globules;
Water, four ounces. A spoonful for each dose. The dose she was ordered to take after each violent retching.

On Sunday night, at ten, p.m., her husband came in great anxiety, wishing me to visit his wife immediately, as they all expected she was dying. On arrival I found that though the previously existing symptoms existed, still they were not *augmented* to the degree that by this time they must have been if she were really worse; I therefore gave hope. Gave three spoonfuls of her ipecacunha mixture and left, in case the exhaustion should increase, three globules of China (cinchona) to take; otherwise to continue the ipecacuanha mixture.

Monday, July 17. She was restless all through the night, till the morning. At four in the morning her mother administered the three globules of China. She then fell asleep, and slept better since that hour than she has since Friday. She took a cup of cocoa and some barley-water, and both remained on the stomach. She has not *vomited* since she took the arsenicum on Saturday. She complained last night of pain in her head, and wandered much; her eyes *not closed* when asleep. She is now hot and thirsty; her forehead also is hot; the retchings have diminished; her bowels have *not* acted; she is restless when awake. I ordered her two mixtures:—

℞ *Aconite*, four globules;
Water, four ounces. Ft. mist., No. 1.
℞ *Nucis vomicæ*, four globules;
Water, four ounces. Ft. mist., No. 2.

I directed that she should take a fourth part of No. 1 mixture at once; wait four hours, then take a fourth part of No. 2; wait six hours, and then repeat as before.

18. Slept still better last night; was not awake more than an hour from twelve to six; her eyes more closed in sleep; she seems still inclined to vomit, but to-day it is rather heaving; water passes freely; bowels *not* opened; she complains of a pain in her stomach and of a fulness. Her hands burned last night, and this evening she is a little feverish. To-day she is decidedly better; her lips are less blanched. She is, however, *more sleepy* to-day. I left three globules of opium, which were ordered to be taken if the bowels remained confined and the restlessness increased, and the eyes half closed, and the tongue should become brown, and the sleep comatose. If all these symptoms do not appear, then to continue the aconite and nux vomica mixtures.

20. Slept well for four hours last night and slept with her eyes *closed;* has had very little retching; tongue not so black; thirst less. She has not as yet eaten anything, but expressed a wish for some stewed eels. *Her bowels still inactive*, though she has felt a desire this morning to relieve them, but without effect; water clear; still pain and a sense of fulness about the stomach, and she cannot bear pressure at the pit of her stomach; she has complained, also, of a severe pain in her back; her temper is peevish; her restlessness, when awake, is much lessened; her lips begin to assume a shade of redness; hands less hot. Has taken some beef-tea. She did not take the opium globules. Prescribed an aconite and a pulsatilla mixture, four globules in each, and ordered a bread-poultice, on which twenty drops of the tincture of pulsatilla, of the first dilution, were dropped, to be applied to the pit of the stomach, and directed, that, should the bowels not act the next day, she might have an injection of warm water.

22. Her improvement is great; all her family and friends are astonished; she sleeps well; lips are more natural; tongue less black; ate a boiled sole yesterday; *bowels still inactive*, though she has had two injections. She has a little pain in the head, and more pains and rumblings in her bowels; the pain in the stomach is gone. I ordered four globules of cocculus (one globule every eight hours, in a wineglass of water), for the inaction of the bowels, the rumblings, and the pain.

23. *Bowels inactive still;* complains of her head; slept well last night, and awoke quite sensible; her colour is returning. She complains of pain in her stomach and bowels and there is some soreness on pressure; and she was directed to continue the *cocculus* till three, p.m., when, if her pains were not better, she was ordered to take *aconite*, one globule, and four hours after one globule of *nux vomica*.

24. Head *better;* she is stronger; eats heartily; took some mutton yesterday; *bowels still inactive;* about four, p.m., felt a wish to relieve the bowels but with no effect; she has severe pain in her back, and some tenderness about the bowels; water free and clear; slept well last night, but had during sleep *one* of her eyes *open;* she has still a little day-restlessness. Ordered one globule of opium in a wineglass of water every eight hours, for the inaction of the bowels and the sleeping with the one eye open.

25. Slept well last night and with the eye closed. After taking three doses of the opium her bowels were freely open; the stools black and offensive; lips are regaining rapidly their natural colour; the fingers have still a marbly hue. Ordered a globule of opium once a day.

31. The patient is able to sit up and to walk about the room without assistance;

she eats heartily, but sometimes brings up her food; the bowels have been confined since the 28th; water clear. The monthly period has not appeared; she has pain in the back of the head and great soreness there,—of these she continually complains. I ordered pulsatilla, one globule, in two spoonfuls of water,—one spoonful twice a day.

Aug. 8. She paid me a visit; she had been at "public worship" on the Sunday. The back of the head is painful when she lies down, and the pain has kept her awake the two last nights. Her food agrees; appetite good; food remains quiet; bowels tolerably regular. The soreness of the back of the head she ascribes to the fall at the commencement of her illness. I ordered *arnica*, and my patient became well.

This case presents several interesting features.

The first is the *inactivity of the bowels*. The bowels did not act for *ten* days, and yet, notwithstanding this inaction, the patient became *steadily better*. I am quite satisfied that this inaction of the bowels was an essential to the cure; and, further, that, had this patient's bowels been *forced* open by *purgative* medicine, hæmorrhage would have recurred, and death must have been the sequel.

A second feature of interest is the evidence afforded in the effect produced by the opium on the bowels, of the power of opium in removing *inaction* of the bowels in certain conditions.

I may add here, that I have cured the most obstinate constipations by opium, in infinitesimal doses; but let it not be supposed, in proffering this statement, that I assert that opium is *the* cure for constipation generally. Opium will cure *the* constipation which is attended with symptoms to which it is homœopathic, *i. e.*, to which the opium has the power of producing in a healthy persons similar symptoms. Those, therefore, who think to cure constipation by the use of opium without first ascertaining whether the concomitant symptoms are similar to those produced by the operation of opium, will be themselves deceived, and will injure their patients.

Great Russell-street, Jan. 10, 1843.

INFLUENCE
OF
MATERNAL IMPRESSIONS ON THE FŒTUS.

To the Editor of THE LANCET.

SIR,—Having seen in THE LANCET of the 6th inst. (page 494) some remarks of Dr. Houston, on the influence of external circumstances on the fœtus in utero, permit me to superadd two cases, which recent observation has presented to my notice; one is that of a poor woman, Ellen Burfries, residing at 22, Apollo-buildings, Lambeth, who, when last *enceinte*, was afflicted by the death of one of her children. From some cause or other the right palpebra of the deceased child remained elevated, exposing the globe of the eye, with dilated pupil, and glassy vacant expression. This produced in the mother a strong feeling and anxious desire to close the eye, which she frequently attempted, though to no purpose. On her subsequent accouchement the child she had displayed the same marked characteristics of the right eye, in which the development of the iris was arrested, as also of the pigmentum nigrum to a considerable extent. There is a collapse about the organ in general, which cannot fail to attract notice.

The other instance to which I allude occurred in a patient of Mr. Miskin, of Bridge-road, Lambeth, to whose kindness I am indebted for the imperfect examination I had an opportunity of making. In this instance the right lower extremity presented externally the appearance of an amputated limb,—a small process of integument, about the size and shape of a pea, being attached to the stump by a pedicle internally. The anus was imperforate, and no person could positively distinguish the sex, the integuments merely exhibiting a purplish hue, and something elevated, approximating to a nævus, with a small cutaneous prolongation, which might be construed into the rudiments of a penis, or, with as much reason, of a clitoris. There were, however, no labiæ or vulva, neither was there a scrotum or perforation of any kind.

The parent would not permit the deformed child to be removed from the room where she lay, but having expressed a desire to know if "the limb" was perfect beneath the skin she permitted a partial dissection. As, however, my operations were likely to be interfered with, I made a section of the lower portion of the femur, and transferred it to my pocket. Having next examined the perineum I found there was no terminal rectum, but a convolution of intestine lying transversely in the direction of the tuberi ischii. Here my investigation was put an end to by the faintness of the mother, who requested me to desist. On reference to the deformed limb, I found that the development of the tibia had been arrested at its superior sixth, that it terminated in a tuberculated point, a little hooked, standing at an obtuse angle with the femur, and that it was dislocated inwards, on the internal condyle of that bone, to which it seemed disposed to become attached.

As regards the history of this case, I found the mother had been the subject of two previous miscarriages, and, whilst crossing the road during her late pregnancy, she stumbled over some gas-fittings, when her *right* knee doubled under her, the calcaneum

coming in juxta-position with the nates; and at this, as well as former periods, her mind was impressed with the fear of miscarriage of an imperfect child. Sexual differences also seemed to attach much to her imagination, as she believed she could miscarry more easily with a girl than a boy; and she *thought, perhaps*, this *might* be a boy. Thus we may trace some connection between the accident she sustained, the reasoning which occupied her mind, and the abnormal appearances of her offspring.

In conclusion, Mr. Editor, without entering on the practical observations which such a case might suggest in an obstetric view, I would take leave to inquire, where there is such a predisposition to miscarry amongst females, with whom it has once occurred, and where mental impressions exert such powerful influence, whether the former be not, in a great measure, the result of a preconceived notion that such will occur? Should such be true it would be an important duty of any practitioner, in similar cases, to establish the confidence of his patient in the happy result of her pregnancy. In the hope that I do not intrude too much on your time and patience, I remain, Sir, your obedient servant,

M. K. O'Shea, M.R.C.S.

Lambeth, Jan. 8, 1844.

FOREIGN BODIES IN THE EAR.

To the Editor of The Lancet.

Sir,—The observations on the extraction of foreign substances from the external meatus of the ear, contained in the admirable practical lectures of Sir Benjamin Brodie, now adorning the pages of your excellent Journal (page 458), bring to my recollection a case which I witnessed some dozen years ago at one of the great metropolitan hospitals. A boy was brought into the operating theatre to have a pea removed from the meatus. The surgeon attempted to extract it with the forceps, and, in so doing, used no inconsiderable violence; indeed, he remarked to the students around him that "some degree of force was *necessary* in these cases." The force employed, however, did not avail; the poor boy was removed from the theatre, with the pea still in his ear. Inflammation supervened, the brain became involved, and in a few days the boy died.

Conversing about this case shortly afterwards with the late Mr. Alcock (to whose memory, as a most accomplished surgeon, I would fain pay a tribute of respect), he showed me a little instrument* which he had lately used on a similar occasion. It consisted of a piece of twisted copper wire, making a handle with a loop at the end, which diverged at a right angle from the handle, or less acutely. This loop, adapted to the size of the passage, and bent to a convenient angle, was insinuated, by a knack of the operator, *beyond*, as it were, the pea, and the extraction was easily effected.

I trouble you with this little communication because I feel that suggestions of a practical nature, however apparently trifling, may, at one time or another, prove of immense service in a profession so fraught with difficulties as is that of surgery. I have the honour to be, Sir, your obedient servant,

Chirurgiculus.

Jan. 8, 1844.

* Mr. Alcock was an expert mechanic, and made many of his instruments with his own hands; he had, consequently, provided himself with a host of contrivances adapted to all the more delicate operations of surgery.

LETTERS ON THE

NERVOUS SYSTEM OF THE GRAVID UTERUS,

Addressed by Messrs. Paget, Lawrence, Stanley, and Skey, Sir Benjamin Brodie and Dr. Gulliver, to Dr. Robert Lee, after examining his Dissections.

To the Editor of The Lancet.

Sir,—May I request of you the favour to print in your Journal the following letters, which have been sent to me by different eminent anatomists who have recently examined my dissections of the nerves of the gravid uterus. I remain, Sir, your obedient servant,

Robert Lee.

Saville-row, Jan. 15, 1844.

1. *From* James Paget, Esq.

St. Bartholomew's Hospital,

Dec. 19, 1843.

My dear Sir,—After the minute examination of your preparations of the nervous system of the gravid uterus, which you permitted me to make, I examined your descriptions and Mr. Perry's delineations of them. Both appear to me remarkable for their accuracy.

With regard to any doubt whether the structures which you have dissected be, as you have described them, nerves and nervous ganglia, it is chiefly by the following facts that I feel convinced of the truth of your account:—

1. That there is a visible and orderly continuity between the well-known hypogastric and sacral nerves, and the most minute and remote of the nervous cords which you have displayed.

2. That these cords, like those of larger size, have such an appearance of being nerves, as could not, I am sure, be imitated by dissections of any other known structures.

3. That the ganglia which you have found are like those of other parts of the sympathe-

tic system, and have distinct nervous cords passing to and from them.

4. That many of the minutest as well as of the larger nerves are evidently associated with blood-vessels.

5. That the nervous system displayed is such an one as the pregnant uterus, from its known structure and functions, might be expected to possess.

I think that your statements are proved by *all the evidence to which it is possible in the present condition of science to attain*; and I beg to add my congratulations to those which you have already received, on your having, by a truly admirable perseverance and dexterity, made one of the most important anatomical discoveries of this century. Believe me, my dear Sir, very truly yours,

JAMES PAGET.

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2. *From* WILLIAM LAWRENCE, Esq., F.R.S.

Whitehall-place, Dec. 28.

My dear Sir,—I have carefully examined your dissections of the nerves of the gravid uterus on various occasions, and I continue to believe, as I did in the first instance, that the structures you have succeeded in displaying are ganglia and nerves, while I am satisfied that your figures and descriptions are perfectly correct. The extraordinary and interesting spectacle you have set before us is so entirely novel, that some hesitation might very probably be felt in arriving at a decided opinion on the subject. Yet this augmented size of nerves and ganglia is only analogous to the increased development of blood vessels in the gravid uterus, with which we have been long familiar; probably a corresponding change might be found in the absorbing system, if it were submitted to the same kind of patient investigation which you have bestowed on the nerves.

The considerations which have led me to form the conclusion above mentioned, are exactly the same as those so clearly stated by my colleague, Mr. Paget; I have therefore only to add that I entirely agree with what he has written to you on the subject, remaining, my dear Sir, yours very faithfully,

WM. LAWRENCE.

Dr. R. Lee.

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3. *From* EDWARD STANLEY, Esq., F.R.S.

Brook-street, Jan. 8.

My dear Sir,—In reply to your question with what impression I came from the examination of your dissections of the gravid uterus, I can have no hesitation in stating my opinion to be, that your dissections *do satisfactorily display the uterine nerves and their ganglia*. Looking to the arrangement, connections, and relations, with the blood-vessels which the filaments and intumescences present, I cannot think otherwise of them than that they are the uterine nerves and ganglia. Believe me to be, my dear Sir, yours sincerely,

EDWARD STANLEY.

Dr. R. Lee.

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4. *From* F. C. SKEY, Esq., F.R.S.

Grosvenor-street, Jan. 6, 1844.

My dear Sir,—I have much pleasure in complying with your request that I would state the result of my impressions on viewing your dissections of the impregnated uterus. I see no reason to doubt the correctness of your views, viz., that the immense network which you have so industriously investigated, on the surface of the uterus, is composed of true nerves and nervous ganglia. To my judgment they present all the visible characters of nerves, and appear to be in direct communication with the pelvic branches of the sympathetic. I am, my dear Sir, very truly yours,

F. C. SKEY.

To Dr. R. Lee.

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5. *From* Sir BENJAMIN BRODIE, Bart., F.R.S.

14, Saville-row, Dec. 29, 1843.

My dear Sir,—I have carefully inspected your preparations exhibiting the nerves of the gravid uterus, and *I can find no reason to doubt the correctness of the statements which you have published* in the "Philosophical Transactions." Yours truly,

B. C. BRODIE.

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6. *From* GEORGE GULLIVER, M.D., F.R.S.

Hyde-park Barracks, Jan. 12, 1844.

My dear Sir,—I have read your papers in the "Philosophical Transactions" on the ganglia and on the other nervous structures of the uterus, and carefully examined the preparations which you exhibit as vouchers for the facts you have announced.

Your descriptions and plates appear to me to be executed with remarkable fidelity.

I cannot doubt that the structures which you have shown by dissection *are really nerves and nervous ganglia*, because their fibres are clearly shown to be continuous with the sympathetic and with the sacral nerves, and because I do not believe that any ingenuity in the art of dissection could show such a regular continuity between the nerves and fibres of elastic tissue, or that this tissue could be made to imitate the nerves and nervous ganglia displayed in your preparations. I am, my dear Sir, very sincerely yours,

GEORGE GULLIVER.

THE fluids absorbed by the spongioles of plants is converted in their interior into a kind of starch, which, being stored up, has an analogy to the fat of animals.—*Dr. Power.*

THE LANCET.

London, Saturday, January 20, 1844.

Our medical brethren on the other side of the channel have no sooner got out of one "medical trial" than we find them seriously engaged in another. This time, however, the subject of debate is by no means of so important a nature as a few weeks ago, when freedom of scientific discussion was at stake; and we should scarcely have thought it worth while to reproduce the "facts of the case," were it not that they illustrate a peculiar feature in the state of modern French society.

It is a singular fact, but nevertheless true, that in France, at the present time, the higher and richer classes are nearly as much in the habit of embalming their departed relatives as were the Egyptians, or the inhabitants of the Canary islands, in the days of old. The principle difference, however, between the men of former times and our continental neighbours is, that the former embalmed their dead in such a manner as to render them nearly imperishable, as our museums can testify, under the impression that the soul remained near the body as long as it retained its terrestial form; whilst the latter, who are not much troubled with superstition, as every one knows, are content to impart to their dead a temporary immunity from decay.

It would be difficult for us to say how long this has been the case in France, or how the custom originated. The inquiry would be an interesting one, but as we have neither the leisure nor the inclination to pursue it, we shall content ourselves with stating that this custom is the key to the trial which now has the privilege of enlivening the French medical public.

Until within the last few years the embalming of the departed relative was always intrusted by the family to the medical man who had been in attendance during the last illness. As professed embalmers, as among the ancient Egyptians, were not known in France, the operation naturally fell into the hands of the members of the medical profession, who, indeed, were nothing loth to attend their former patient to the last, the more so as their labours were always liberally remunerated. Such a state of things, however, could not be expected to last for ever, and a few years ago a formidable competitor arose in the person of M. Gannal.

The efforts of M. Gannal were at first merely directed to the conservation of subjects for anatomical purposes. In the year 1834, he read before the Academy of Sciences, and the Academy of Medicine, a memoir containing the result of his researches. He therein proposed, for the preservation of dead bodies, to soak them for several weeks in a bath containing twenty pounds of chloride of sodium, twenty of sulphate of alum and potash, and ten of nitrate of potash. Some time afterwards, in order to attain the same end, he proposed to inject a solution of sulphate of alumina and of arsenic into the arterial system, by way of the left carotid, and in September, 1837, he took out a patent for what he called "his invention."

It soon became evident that M. Gannal, dissatisfied with the unproductive laurels he had hitherto culled, was determined to turn his researches to account by laying an embargo on the bodies of those whom the faculty had failed to snatch from the jaws of the grim tyrant. In the course of a few weeks after the date of the patent, his portrait, with his address at the bottom, was to be seen in every printseller's window in Paris, and that city was deluged with advertisements and bills, announcing to the world the marvellous powers possessed by M. Gannal to preserve the human shape "divine" from decay. Since then, as might be expected, M. Gannal, has been at war with the medical men. Thanks to his indefatigable vigilance, nearly every illustrious death that has since occurred has been the means of bringing him and his claims before the public,

sometimes as the happy, self-satisfied embalmer, at other times as the disappointed candidate, and the bitter critic of the performance of his more dignified opponents. M. GANNAL's ambition gradually increased, however, as his success became greater, until at last it knew no bounds. In his own mind he became the personification of embalming, and any such operation performed by another he looked upon as an invasion of his dominions, and an invasion which his "patent" enabled him to repel.

Under the influence of these ideas he, last year, brought an action against a Dr. BROUARD, who had dared to embalm one of his clients, and he gained the cause before the Tribunal of Auxerre, though after having previously lost it before that of Sens. Elated with this triumph M. GANNAL immediately announced that, he alone having the right to embalm by injection, he should prosecute any medical man who attempted to infringe on his patent.

This decision, so contrary to the interests of medical practitioners, and, as they say, so contrary to the dignity of the profession, has induced M. MARCHAL, a young and distinguished Parisian surgeon, and editor of one of the medical journals, to come forward as the champion of the followers of ESCULAPIUS. M. MARCHAL, a couple of months ago, by the left carotid, injected publicly a body with a solution of arsenic, having previously given M. GANNAL notice of his intended operation.

Thence the present trial. M. GANNAL has brought an action against M. MARCHAL, before the "Tribunal Correctional" of Paris, and several sittings have already been devoted to the debates. M. MARCHAL, following the example recently set him by M. MALGAIGNE, conducts his own case; and that, it is said, with such eloquence and skill as much to have added to his reputation. M. GANNAL's claims rest on his communication to the Academy of Sciences in 1834, on two prizes he obtained from that Academy in 1835 and 1836, and on his patent for embalming by injection. M. MARCHAL's objections are numerous and weighty. We will briefly analyse them. M. GANNAL, he says, obtained his prizes from the Academy for his system of preserving bodies in a saline solution, and not for his mode of injecting by the carotids. The injection of a preservative fluid into the arterial system, for which he obtained his patent in 1837, was known long before that time. RUYSCH preserved bodies by injection; BERZELIUS in 1823, LAUTH in 1829, described the method of preservation by injection; and in 1835 M. TRANCHINA gave, in an Italian journal, a new method of embalming by the injection of arsenic into the left carotid. Moreover, the incision of the skin and carotid, even on the dead, is a surgical operation or dissection, for which a patent cannot be given; M. MARCHAL, also, used a solution of arsenic only, whereas M. GANNAL's fluid is a mixture of arsenic and sulphate of alumina.

Good sense declares that the medical men are right,—that all operations on the dead, as well as the living, belong to those who alone are acquainted with the structure of the human fabric. Setting aside all considerations of profit, we think that decency requires that all surgical manipulations on the dead, which the feelings of the survivors may render necessary, ought to be intrusted to the profession, and not thrown by a patent into the hands of mercantile dabblers in anatomy, whose rapacity induces them to intrude on the privacy of the living, and on the death-chamber of the deceased, in order to press their claims to notice.

When the trial is concluded we will inform our readers of the decision of the Paris judges, which will, we hope, be such as to meet with the sympathy of all those who wish to preserve intact the dignity of our profession.

IN his remarks on auscultation, in THE LANCET of January 6, Dr. H. BENNET misquotes our leading article in the number for Nov. 25, of last year. Referring to our observations he says, "As an experienced "auscultator, I cannot possibly admit 'that

"'every sound yielded by auscultation is, "'to a well-trained ear, as distinct, though "'not so loud, as the stroke of St. Paul's "'bell.'" Far be it from us to make any such assertion, for we know full well that many a sign yielded, or said to be yielded, by auscultation, is but "the viewless spirit of a lovely sound," among the attributes of which distinctness is anything but prominent. Such sounds as these we recommend to the disciples of HAHNEMANN, as appropriate allies in their contests with the ghosts of diseases. Our words were these,—"Every "sound yielded by auscultation, that is "worth one straw of confidence, is, to a well-"organised and well-trained ear, as distinct, "although not so loud, as the stroke of St. "Paul's bell." And, as "experienced auscultators," we adhere strictly to our position. We affirm that a sound, with reference to the existence of which there is any doubt in the mind of a competent auscultator, ought not to be admitted among the elements of his diagnosis. Such a sound may suggest a suspicion to his own mind, and may, therefore, indicate the expediency of frequently-repeated auscultation, to find if he cannot recognise it more distinctly; for if he allow a sound which he cannot clearly make out to influence directly the diagnosis he delivers, he will stand a round chance of getting himself into hot water, and of bringing ridicule on the useful practice of auscultation. The remarks of Dr. BENNET on the relative merits of mediate and immediate auscultation, may probably induce us to return to the subject on a future occasion.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, January 9, 1844.

EDWARD STANLEY, F.R.S., President.

Case of the Removal of a Diseased Ovarium, terminating fatally on the seventh day after the operation. By T. M. GREENHOW, Esq., Surgeon to the Newcastle Infirmary, &c. Communicated by Sir B. C. BRODIE, Bart.

THE patient was 29 years of age, and married. For four years she had suffered from frequent discharges of blood from the uterus. Eighteen months ago, six months after her marriage, the swelling in the abdomen commenced at the pubic region, and rapidly increased till it attained a large size, her strength all the while declining, from the constant uterine discharge. The abdomen was tapped; only a little blood escaped, but afterwards nearly a quart of dark-coloured fluid was discharged from the wound daily, for about a fortnight.

Before the operation of removing the tumour, which was performed on September the 3rd, the abdomen was about as large as at the full period of utero-gestation; there was fluctuation in one or two parts, but the tumour generally was firm, and felt as though divided into separate masses. The incision reached from a little below the ensiform cartilage to near the pubis. Several adhesions existed in different parts, the principal one being to the omentum, which was spread over the upper part of the right side of the tumour. These adhesions were divided with the bistoury, and then the tumour was raised, with some effort, owing to its great size and weight, and from its situation; double ligatures were passed through its pedicle and firmly tied, and this part being divided near the tumour, it was liberated from its attachments and removed. Two arteries bled freely, one in the divided omentum and the other in the pedicle; upon these being secured, the wound was brought together by sutures and adhesive plaster, and a bandage applied. The operation was well borne by the patient, although she vomited several times towards the end; the quantity of blood lost did not exceed six ounces. The symptoms which followed were chiefly great retching and vomiting; constipation of the bowels; quick pulse; tenderness and distention of the abdomen; and she died on the morning of the seventh day.

On the post-mortem examination the folds of the intestines and the omentum were found glued together by recently effused lymph. There was inflammation, with points of ulceration, near the pyloric orifice of the stomach. The uterus was healthy, but its cavities were lined with a vascular membrane, like the decidua. The morbid growth had been attached to the left broad ligament. On examining the tumour it was found to weigh twelve pounds seven ounces, and to be more than two feet in circumference. The surface was smooth and of a pale colour, resembling that of the skin. With the exception of a few cysts containing a yellow fluid, the general mass was composed of a dense and vascular cellular structure, with a membrane of small cells or cysts pervading its substance. The author concluded by making some remarks on the case. He directed attention particularly to the disease found in the stomach, which he considered had an important influence in leading to the fatal termination, and thought, on the whole,

that the result of the case did not offer a strong argument against the resorting to a similar mode of treatment in such cases.

Case of Extirpation of an Ovarian Cyst. By BRANSBY B. COOPER, F.R.S., Surgeon to Guy's Hospital.

The patient, aged 32, was married for four years without having children. She had suffered at different periods from dysmenorrhœa and leucorrhœa. Five years before her admission her abdomen became enlarged, and having consulted the author, he considered the case one of ovarian tumour, and proposed to draw off the fluid from the cyst by a small opening in the abdomen; but this plan was not carried into effect. She was tapped on two different occasions, and about three gallons of straw-coloured fluid were drawn off each time. When she applied at the hospital, earnestly desiring to have the operation performed, the abdomen measured about three feet and a half in circumference. Mr. Cooper resolved to perform the major operation. An incision was, therefore, made through the abdominal parietes, from the ensiform cartilage to the pubes. A few adhesions to the tumour were met with near the point where the trocar had entered in tapping; these having been divided, the cyst was dislodged and brought out through the wound, when the pedicle, connected with the right ovary, came into view. A double ligature was passed through the pedicle, and both threads having been tied, the pedicle was divided between the cyst and the ligatures. The left ovary was examined and found to be healthy. The wound was then brought together by sutures and strapping, and a roller applied round the abdomen. There were some attempts to vomit during the application of the ligatures, but otherwise the patient bore the operation well.

A minute record was given of the daily condition of the patient subsequently to the operation, but it is sufficient to say that distinct symptoms of peritonitis soon appeared, and the patient died on the seventh day, without any indication of the system rallying.

On the after-death examination, lymph was found effused extensively over the abdominal parietes and viscera, uniting them in many places. A small piece of omentum was enclosed in the ligature applied to the pedicle. The uterus was large, tumid, and of a dark colour, and a soft fungous tubercle, pronounced, after microscopical examination, to be malignant in its structure, was found at its fundus. The cyst which was removed was of an oval form, and at its superior and anterior part there was a collection of compound cells. The weight was thirty-two pounds. The author concluded by making some general remarks on the operation. He drew attention to the peculiar kind of inflammation which followed, and was inclined to think that it was excited more by the presence of the ligature applied to the pedicle, and left in the wound, than to the extensive incision. He also noticed the fact of a tumour of a fungoid structure having been found growing from the substance of the uterus, and thought that this was interesting as illustrating the views of those pathologists who believe ovarian tumours, consisting of cysts, to be malignant in their nature, and as bearing on the question of the propriety of removing such tumours by operation.

After the reading of this paper,

Mr. BRANSBY COOPER, seeing Mr. Lawrence present, and knowing that he had some decided opinions respecting the operation, requested the favour of his communication of them to the society.

Mr. LAWRENCE rose and said that it was quite true that he had strong opinions respecting the operation in question, but they were opinions founded on no personal experience respecting it, for he had never even had an idea of performing it, and should strongly advise a patient, if his advice were asked on the subject, not to submit to it. Among the many points which presented themselves to us in forming an estimate as to the propriety of ranking this among the regular operations of surgery, or of performing it, if requested by a patient to do so, he should select two:—First, as to the nature of the operation itself. He was fully aware that great difference of opinion existed among surgeons with regard to the danger of the operation; the papers just read had proved this. Some considered it a slight matter to make an incision into the abdomen, extending from the ensiform cartilage to the pubis, turning out a tumour weighing thirty or forty pounds, breaking up adhesions that it might have formed, and applying ligatures to the abdominal vessels; but he looked upon it as a very serious matter, and believed that the results were so formidable that it was an equal chance whether the patient lived or died. Besides, we were not yet in possession of sufficient statistical data upon which to found a decided opinion; for whilst favourable and successful cases had been published with wonderful alacrity, the unsuccessful instances of the operation had been kept studiously and carefully from the public eye. Such being the case, we were not in a condition to strike a fair balance; but were we fairly in a condition to put the "contra" against the "credit" side of the account, he doubted not that we should decide that the operation was dangerous and terrible. If there were to be found any justification for this operation, it would be an expectation that its performance would relieve great suffering, and prolong the life of the patient. We knew that some persons so cling to life as, when there was great and urgent danger, to be willing to submit to

any operation. This consideration brought him to the second point. Was ovarian disease usually of so painful and urgent a nature? His experience taught him to answer in the negative. Patients so afflicted usually lived comfortably for years, resorting only to occasional tapping to relieve the bulk and weight of the collected fluid. Tapping often relieved them for a very considerable time. He did not at all agree with Mr. Cooper in thinking that it afforded a mere temporary relief in all cases. Indeed, he could call to mind instances in which it had effected a cure. He knew the case of a lady, whom he first tapped twenty years ago, and though she had only submitted to the operation twice since, you would not now know that there was any ovarian disease whatever. In another case in which, many years since, he had drawn off a moderate-sized pailful of fluid from an ovarian cyst, and repeated the operation two or three times at intervals of three or four years, the patient got quite well. In addition to these occasionally very successful effects of paracentesis, it must not be forgotten that ovarian cysts occasionally burst into the abdominal cavity, and the fluid became absorbed. Even, indeed, in cases less favourable than either of the two cases related, or than those in which the cyst was ruptured, the life of the patient was prolonged by tapping, for five or six years, on the average. Looking, then, at all these circumstances, he was fully of opinion that the operation for the removal of ovarian tumours was not a fit one to be entered into the catalogue of surgery. (Applause.)

Mr. B. Cooper observed that Mr. Lawrence had been particularly fortunate in his performance of the operation of paracentesis. He feared, however, that the cases related were only exceptions to the general rule. At all events, his own experience on the subject was brightened by no such instances of success, for out of twenty cases in which he had performed it, in no one single instance did a cure result. He did not regard tapping as a slight operation, and would not resort to it except in case of absolute necessity. The ovarian disease gave little inconvenience to the patients until the accumulation produced constipation or vomiting, but after paracentesis he had noticed that they often became quite ill, and a burden to themselves and others. The operation for the removal of ovarian cysts by incision was a formidable one, but by no means difficult to perform. With respect to Mr. Greenhow's case, he thought its history, the slight fluctuation in, and the position of the tumour, did not justify the operation. The nature of the tumour, as evinced in the drawings before the society, proved this to be the case, for instead of being translucent, as ovarian cysts usually were, this was of a solid character. With respect to another point, the relative advantages of the major and minor operations, he was fully of opinion that the minor was the better of the two. He had performed the larger operation in the case before the society, because he had seen instances in which the performance of that operation by Mr. Walne had proved successful, and he thought it a duty not to be biassed by any feelings or prejudices on the matter, but to give the patient the benefit of the experience of other surgeons. Had he (Mr. Cooper) another operation to perform, he should make an incision of about four inches in extent, draw the fluid partially off, but leave sufficient in the cyst to enable him to get at the pedicle and remove the growth entire. He thought the result of Dr. Frederic Bird's two cases, the only two in which he had operated, proved that the minor operation was the more advisable; for even if there were adhesions, and the examination could not be carried sufficiently far by means of the small incision, this could be enlarged to any extent that was found necessary. He (Mr. Cooper) was not an advocate for the operation, and should never propose it in any case; in the one related to the society the patient herself had urgently requested him to perform it, and he acceded to her request. If, however, the statistical accounts were to be relied on, he did think the operation might be justifiably performed in some cases.

Dr. Merriman spoke very low; we understood him to say that in Mr. Greenhow's case, the presence of leucorrhœa and menorrhagia showed such a state of disease of the genital organs as would make it little likely to be relieved by operation. The state of the uterus might and should have been ascertained before the knife was resorted to. Even in Mr. Cooper's case the post-mortem examination revealed disease of the uterus. [Mr. Cooper—But Dr. Blundell couldn't detect it during life.] But there it was, and the case could not have been quite successful. Dr. Merriman then made some remarks on the very deficient state of our statistical knowledge on the subject. We were not in possession of all the facts we might be, for unsuccessful cases were held back. He related a case of ovarian dropsy in which the patient had borne two children after paracentesis had been performed.

Mr. B. Phillips spoke from some little personal experience on the subject, having been engaged or present in four cases in which the operation had been performed. In two of these it was quite successful. In one it gave rise to some after trouble; and in another, in which he had performed the operation himself, the termination had been unfortunate. He did not at all agree with Mr. Lawrence that the magnitude of the operation was any reason whatever against it taking rank among the other operations of surgery, for many operations of equal magni-

tude were resorted to. What we wished to know, and what, indeed, was essential to be known, before we could arrive at an accurate conclusion respecting it, was the actual result of the whole of the operations already performed. This we did not know. With respect to the disease for which the operation had been resorted to, if we could draw anything like a middle course, the case would be different. But could we do so? The results of treating the disease were not satisfactory, and we delayed tapping as long as possible. When it did become necessary to take some step, then we might consider whether it would be advisable that the operation of paracentesis or that of extirpation should be resorted to. In coming to a conclusion on this point it must be borne in mind that paracentesis was occasionally followed by a fatal result, and that though some cases of ovarian disease, like those related by Mr. Lawrence, might stand still after the operation, the majority, as was well known, would go on. We could not, even in the most favourable cases, expect a cure to follow a single tapping, although such had occasionally occurred. Generally speaking, four years after the first tapping terminated the case. If, then, we had a young woman under our care, in whose case it was imperative that something should be done, would you tap her, running the risks attendant upon the ultimate success of that operation, or would you perform an operation like that of extirpation, after which, if it were successful, the patient might live for many years? We were still, he thought, without sufficient statistical information to guide us in the matter. When he performed the operation he had been acquainted with the cases of Jeaffreson, King, and West, and thought favourably of it. He communicated to the patient the dangers and chances of the operation, but did not advise her to submit to it, and was only induced to perform it at her urgent request, and with the assurance that if he did not do it some one else would. The case was unsuccessful; but, contrary to what now appeared to be the fashion, he immediately published it, because he thought, in a new operation, such as this was, it was most desirable for the surgeon to know all that could be known respecting it, when advising with a patient. He considered it a great dereliction of duty in any man who withheld unsuccessful cases from publication, and he feared the remarks of Mr. Lawrence on this point were but too true, and that several unsuccessful cases had not yet seen the light. With respect to the kind of operation to be performed, he was an advocate for as small an incision as was consistent with the removal of the tumour. Why should we expose all the cavity of the abdomen when it was not at all necessary? In support of his preference for the *small* incision, and of the fact of its being sufficient for all purposes connected with the proceeding, he would adduce three cases in which it had been performed, and tumours as large as that exhibited to the society removed. In his own case the cyst contained 335 ounces of fluid. In one of Dr. Bird's cases it contained a bucketful, and in this instance, as well as in the other case operated upon by that gentleman, the patients were well in a fortnight. The *small* incision had been found adequate for all purposes.

Mr. LAWRENCE had not condemned the operation completely, but, from what he knew respecting it, he would not recommend its performance. He could, however, believe it possible that there might be cases in which it would be advisable to perform it at the request of the patient. So little time, however, had elapsed since the performance of these vaunted operations,—not six, eight, or twelve months,—that we could not tell how far they were successful; but we did know that patients lived years after tapping. Some time ago Mr. Lizars had extirpated an ovarian tumour, and he (Mr. Lawrence) had lately had an opportunity of examining the patient. The operation had been the major, or maximum one, as it might be called. There was a cicatrix extending from the ensiform cartilage to the pubis; he did not know what had been taken away, but the pelvic viscera formed a solid mass. In this case, the intention of the operation, that of being a perfect and permanent remedy, had not been fulfilled, as the disease would terminate fatally. He doubted, then, if it was true that the remedy was a full and permanent one, the only ground upon which it could be recommended. There were other elements of importance in forming our estimate of this operation, not the least of which were the facts alluded to in Mr. Cooper's paper respecting ovarian disease, as illustrated by preparations at Guy's. It was found that, out of fifty cases, in fourteen the opposite ovarium was diseased, and in eight other cases there was malignant disease in some other part of the body. How, then, were we to estimate the real value of the operation unless time were given for that purpose. He agreed with the other speakers as to the preference they had shown for the minor operation; he should never think of ripping up the entire abdomen, unless he could not effect his purpose without; and he thought an opening sufficient to expose and remove the cysts need not exceed a few inches in extent. There was a preparation at Bartholomew's of a cyst which had contained twelve pints, which the late Mr. King, of Saxmundham, had removed by a small opening. He first let off the fluid, then tied the pedicle, and removed the growth. In the remarks appended to some of the lately published cases they were spoken of as favourable ones for the operation. Now these were just the

kind of cases in which we did not require the operation—tapping produced comfort in these. It was in unfavourable cases that we wished for a permanent and complete remedy.

Sir G. LEFEVERE inquired of practitioners whether they had found paracentesis most successful when resorted to early or late in the disease. He related a case which had been operated upon early, and in which it had not returned at the end of twenty-three years.

Mr. DAVIES had tapped a lady fifteen years ago. She was now seventy-two, and had been tapped five or six times since the first.

Mr. LLOYD related two cases in which tapping had been successful in effecting a cure. In one case the operation was twice performed; the patient had a healthy child afterwards. In another case the patient had been tapped thirty-eight times, the last time seven years since, and she was now well.

Dr. COPLAND had paid attention to the subject of the discussion for twenty-five years past. Some most experienced physicians were averse even to tapping in the complaint, among whom might be mentioned the late Dr. Hamilton, of Edinburgh, who considered it usually did harm, and who had found the most successful treatment that consisting of external and internal remedies. Other physicians of eminence were against tapping. He (Dr. Copland) could enumerate several cases of ovarian disease, which had been under his own care, in which tapping had not been resorted to. Some of these cases had remained stationary for years, some had retrograded, some patients had been made comfortable and had scarcely any tumour whatever. In these cases medicines only were employed, no operation having been resorted to. The cases which had proved most successful by operation were those most amenable to medical treatment, not requiring even paracentesis. He should usually, in cases of this disease, trust to nature and change of climate and constitution.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Jan. 13, 1844.

Dr. REID, President.

DISEASE OF THE EAR.

MR. STREETER related a case of disease of the ear, proceeding and terminating in a manner, out of the usual course of such affections. The patient was a lady, forty-two years of age, and had been deaf with the right ear ever since the age of seven, but from what cause was not known. Two or three months ago she became affected with a severe headach, for which a blister behind the affected organ was applied. Nothing, however, was thought of the matter until the 17th of December last, when the pain increased very much in severity, being of a maddening character, and almost inducing delirium. The portio dura was paralysed, as was evident on the patient's being requested to put out her tongue. There was a severe pain, also, down the spine, which was attributed to a fall which she received on getting out of bed. The pulse did not warrant active depletion, but two or three leeches were applied behind the ear, a large poultice over the face, the ear was gently syringed with some warm water, and saline medicines were ordered. On the 18th she had had some sleep in the night, but complained of an almost intolerable pain in her back. The catamenia now appeared, and the cause of the pain remained obscure. She was quite sensible, the pupils acted, but the cornea on the affected side had commenced to ulcerate. The opposite ear had become a little deaf, and there was a slight discharge from the right ear. It was, also, thought that a hole could be perceived in the membrana tympani. Calomel and opium were administered. The following day (the 19th) she had slept better, and remained somewhat improved until five or six o'clock in the evening of the 21st, when she was suddenly seized with coma, in which state she remained until two the following morning, when she died. On examining the brain there were found slight sub-arachnoid effusion and vascularity of the surface of the hemispheres. There was some increase of general vascularity of the interior of the brain. No effusion in the ventricles. An abscess was found in the tympanum and labyrinth, and there was a counter-abscess, about the size of a large pea, in the condensed arachnoid and pia matral tissues, occupying the fossa where the facial and auditory nerves come off from the junction of the medulla oblongata with the pons varolii and cerebellum. There were inflammation and thickening, from effusion of fibrine, of the arachnoid and pia mater investing the right side of the upper portion of the medulla oblongata and adjoining part of the right lobe of the cerebellum, to about the extent of a square inch, but there was no softening nor apparent lesion of the proper cerebral tissues beneath.

Mr. Streeter offered the following explanation of the case:—The inflammation had apparently extended from the labyrinth along the neurilema and the investiture of the seventh pair inwards, by the meatus internus, to the proper investing membrane of the medulla oblongata. The external and internal abscesses had no communication. The petrous portion of the temporal bone presented evident marks of inflammation.

In the discussion which followed the case several points were touched upon, but not such as strictly bore upon the question. Cases were cited to show the equivocal nature of brain symptoms in general.

FOREIGN DEPARTMENT.

ROYAL ACADEMY OF MEDICINE, PARIS.

Among the proceedings of this body in November last, we find a report of MM. Lagneau, Gimelle, and Berard, on several surgical cases, addressed to the Academy by M. Lépine, surgeon to the Hospital of Chalons sur-Saône.

WOUND OF THE LEFT HYPOCHONDRIUM.—ESCAPE OF THE STOMACH.—ILLUSTRATION OF THE TRUE MECHANISM OF VOMITING.

This case was one of a penetrating wound of the abdomen, followed by escape of the stomach, the transverse colon, and the epiploon, cured in twenty-one days.

A labourer, endeavouring to place a yoke on the head of a bull, received several wounds in the abdomen from his horns. One of these, about eight inches in length, occupied the left superior lateral region of the abdomen, following the margin of the false ribs and of the last true one, as far as the xiphoid appendix. The patient was able to return home on foot, a distance of about a hundred feet; on his way he in vain endeavoured to vomit. M. Lépine arrived about two hours after the accident, and found that the wound allowed the stomach, enormously distended, to escape, as also the omentum and transverse colon. The stomach appeared strangulated by the wound; some of its veins were swollen to the size of a common quill.

The first thing done by the surgeon was to return the protruded parts. The reduction of the colon was attempted and effected, although with some difficulty, owing to continued nausea. M. Lépine then applied both hands on the large curvature of the stomach, without being able, however, to circumscribe it entirely, and endeavoured, by pressure, to return a portion of the gases which distended the organ. For a long time the efforts made by the patient to vomit, efforts which were repeated at very short intervals, prevented the reduction; as soon as a portion of the stomach had been returned, the spasmodic contraction of the diaphragm and of the abdominal muscles overcame the resistance offered by the hands of the operator, and re-expelled the part. At last, by perseverance and gentle pressure, the reduction of the stomach was accomplished, and that of the omentum soon followed.

During all the time that the stomach was out of the abdomen M. Lépine neither saw nor felt it contract, although, in order to give rise to contraction, he immersed his hands in cold water previously to placing them on it. The reduction had scarcely been accomplished when, to the nausea and vain efforts to vomit which had existed since the accident, succeeded true vomiting, which relieved the stomach of a quantity of food that the patient had taken half an hour before receiving the wound.

We have not spoken of the other less severe wounds which occupied the abdominal parietes. One of them, nevertheless, is worth noticing; the horn had torn the teguments at the level of one of the external inguinal rings, and, following the inguinal canal, penetrated to the peritoneum. The intestines were seen at the bottom of this wound, which presented also to view the spermatic cord, quite denuded.

The margins of the solution of continuity which had allowed the stomach to escape, were brought together by means of the quilled suture; and a fold of linen was placed in the wound of the inguinal region.

The results of the wound were not serious. The patient only experienced slight pains, which gave way after two bleedings; he had scarcely any fever. A slight swelling of the lips of the wound, which supervened forty-eight hours after the operation, obliged M. Lépine to loosen the sutures, which were only definitively withdrawn on the sixteenth day. The wounds were all cicatrised, and the cure was complete, on the twenty-first day. Since then (1825) unto the present time, the patient has remained perfectly well.

Remarks.—This interesting case not only bears directly on the mechanism of vomiting, but presents several remarkable features, to which we beg to draw the attention of the Academy:—1. The escape of the stomach from a wound of the abdomen is very rare. 2. The difficulty experienced in reducing the organ being attributable to its extreme distention by gases, was not puncture with a small trocar indicated? 3. The success of the quilled suture proves the groundlessness of the fears entertained by some surgeons, and more especially by the illustrious Larrey, whom we lost last year, with reference to the use of this suture in wounds of the abdomen. 4. The non-appearance, subsequently, of hernia, although the patient has never worn a bandage, is calculated to inspire doubts as to the danger of hernia, which, according to some surgeons, is much to be feared after wounds dividing the entire thickness of the abdominal parietes.

M. Lépine judiciously remarks that a more favourable opportunity of appreciating the part which the stomach performs in vomiting could not present itself. The stomach, filled with aliments recently ingested, escapes from the cavity of the abdomen, and the want to vomit manifests itself spontaneously. The phenomena observed by M. Lépine confirm the result of the experiments made by MM. Beclard and Magendie. During all the period that the

stomach remained out of the abdominal cavity there was no apparent contraction of the muscular fibres of the organ, and none of its contents were expelled, although the patient made violent efforts to vomit. As soon, however, as the stomach had been returned into the abdomen, the same efforts were followed by the expulsion of its contents. This fact, as M. Lépine observes, shows that if the stomach be not entirely passive during the act of vomiting, at all events, the most important part is performed by the diaphragm and the abdominal muscles.

M. Lépine was also able to observe a phenomenon noticed by M. Magendie in his experiments on animals, and which he supposed likewise to exist in man. It seems that while vomiting, animals swallow a considerable quantity of air. Speaking of the enormous dilatation of the stomach by gases, M. Lépine remarks:—I can only explain this distention by the air which the patient appeared to swallow after each effort of vomiting; I then observed him to perform repeatedly the act of deglutition, each deglutition being accompanied by a noise which seemed to be created by the pushing back of air.

NECROSIS OF A PORTION OF THE FRONTAL AND PARIETAL BONES.—PURULENT COLLECTION IN THE RIGHT HEMISPHERE OF THE BRAIN.—HEMIPLEGIA ON THE RIGHT SIDE.

This case presents great interest with reference to the relation which existed between the seat of the lesion of the brain and the parts of the body which were struck with paralysis. A man receives a blow on the right temporal region; several months afterwards he is seized with cephalalgia, and presents a puffy state of the integuments of the right temporal region, of the right cheek, and right ear. The functions of the brain become disturbed, and paralysis attacks the right arm and leg. An abscess forms on a level with the temporo-maxillary articulation; is opened, and the patient experiences temporary relief. Very soon, however, he falls into a still more serious state of prostration. His intellectual faculties entirely give way, the paralysis becomes complete on the right side, and he succumbs two months after the first apparition of the morbid symptoms.

The following are the lesions revealed by *the autopsy*:—Perforation of the bones of the skull at the level of the anterior and inferior angle of the right parietal. The internal lamella of the frontal and parietal is separated from the dura mater for an extent of two square inches, and is necrosed and perforated in several places; the dura mater is healthy, except a little redness and increased thickness opposite the lesion of the bones. The consistency of the brain is natural; its surface is not diseased in the region corresponding to the alteration of the bones of the skull. "The *right* hemisphere contained a considerable collection of healthy thick pus, which escaped as soon as the instrument had penetrated the cavity in which it was contained."

This last sentence is literally reproduced from Lépine's account of the case. We are sorry that he does not enter into greater details, as it is of great importance to know in what part of the hemisphere the abscess was situated, whether it was in the anterior, middle, or posterior lobe. Whatever may have been its situation, it is evident that the lesion of the brain was on the right side, and that the paralysis attacked the arm and leg on the same side. M. Lépine, in mentioning this circumstance, remarks how unusual it is. All pathologists have, indeed, noticed the opposition which exists between the side of the head which is wounded and the side of the body which is paralysed. Hippocrates himself states that those who are wounded on the right side become impotent on the left, and *vice versâ*. The authors who followed, although they recognised the correctness of this remark in a general sense, having sometimes observed the contrary, erroneously concluded that the side of the paralysis is not in relation with the local lesion of the brain. This error was owing to the situation of the external lesion being the one taken into consideration and compared to the paralysed side; whereas the seat of the internal cerebral lesion ought to have been the one considered. Nevertheless, cases which we owe to Morgagni and Lancisi, as also several observed in our own times by MM. Bayle and Dechambre, would seem to indicate that the paralysis may occupy the same side as the organic alteration. We must not forget, however, to recall to mind that these were cases of softening of the brain, and not of traumatic effusion. We know only one case, that of M. Blandin, in which the paralysis was on the same side as a traumatic effusion. Henceforth we shall have to add to this case the one narrated above to the French Academy. How can we explain facts so exceptional as these? Can we admit the explanation given by Gall and Spurzheim, and say, with them, that the presence of the cerebral lesion and of the paralysis on the same side is owing to the absence of decussation of the olivary fasciculi of the spinal cord? This question requires elucidating.

PERFORATION OF THE ILEUM
COMMENCING ON THE
PERITONEAL SURFACE,

REPORTED BY DR. ADAMS, OF GLASGOW.

WILLIAM MITCHELL, aged 54, tolerably robust, temperate, and enjoying good health up to May 28, 1840. At ten o'clock, a.m., of that day, I was sent for, and found him affected with all the symptoms of strangulated hernia,—his scrotum filled on both sides with intestine, the left protrusion irreducible. The taxis was successful, and the urgent symptoms almost immediately relieved. At my next visit he complained much of pain in the left iliac region, which a few leeches, followed by warm fomentations, abated considerably. I heard no more of him until Saturday, the 13th of June, and at ten, p.m., on that day I was again summoned. He had enjoyed good health from the time of my last visit, thirteen days before, up to half an hour prior to the present one; however, the tenderness in the left iliac region had never entirely subsided, and was sufficiently acute to prevent his wearing other than his old bag-truss. He had been unable, also, to reduce, as formerly, the entire hernial protrusion, but that circumstance caused him no uneasiness, and his bowels were easily enough regulated. About half an hour before my visit he had, whilst indulging in the company of some friends, been suddenly seized with a violent pain in the left iliac region, accompanied with an extreme sense of weakness, followed by sickness and violent retching. At my visit the pain had become more diffused over the lower part of the abdomen, which was painful to the touch; the inferior portions of the abdominal muscles were so contracted as to feel as hard as a board; the expression of countenance was exceedingly anxious, and the features were greatly altered in appearance; the whole surface of the body was cold, and covered with a profuse perspiration; the pulse was 120, small, and thready; the sickness and vomiting still continued. With respect to the herniæ, both protruded considerably, but could be returned with tolerable facility; excepting a doughy mass, which had been felt in the left hernial sac, even after it was emptied of the greater portion of its contents. From the large size of the abdominal aperture, from the ease with which the greater portion of the hernia could be reduced, and from the statement of the patient, that it had never been more so since my last visit, I was satisfied that there was no strangulation of intestine to account for the symptoms then present. I accordingly diagnosed peritonitis from perforation of intestine. The remedial measures were turpentine embrocations to the abdomen, and, every third hour, three grains of calomel and one of opium. At ten o'clock next morning the patient's appearance was highly satisfactory. He had slept a little during the night; the expression of his countenance was improved; there was less abdominal tenderness; the surface of the body was warmer; the pulse somewhat fuller and less frequent; his bowels were still, however, unopened, and his efforts at micturition were frequent, unsatisfactory, and accompanied with excessive pain. The calomel and opium, with the turpentine embrocations, were continued, and the catheter was used. The appearances of amendment in the patient's condition proved fallacious. At two in the afternoon, when I again saw him, he was evidently moribund. The efforts at vomiting had returned shortly after the last visit, and still continued; the pulse was imperceptible at the wrist; cold sweats again bedewed the body; the countenance was anxious in the extreme; the breathing panting, and accompanied with the tracheal rattle. The patient, who was quite sensible, said he had no pain. He died at six o'clock the same evening. Thus, the time which elapsed between the last sudden attack and its fatal termination was exactly twenty and a half hours.

Examination Thirty-eight Hours after Death.

The body was plump, on many places showed livid patches, and exhaled a most offensive odour; the abdomen was very tympanitic, and the scrotum inflated to an enormous extent; on laying open the abdomen fetid air gushed out in great abundance; the large omentum was much upon the stretch, and at one point adhered strongly to the left hernial sac; on turning it over, the small intestines were seen highly injected, streaked with lymph, and in many places covered with purulent matter; they did not, however, adhere to each other, nor was any portion of them engaged in either hernial sac. In the pelvis, and the right and left lumbar regions, we found a considerable quantity of a dark-brown coloured fluid, having a most offensive feculent smell; and on examining more minutely the state of the intestines, we found, in addition to the general effects of acute inflammation already described, towards the left side, and in that portion of intestine usually considered as the commencement of the ileum, a circular perforation, large enough to admit the point of the forefinger. The appearances of inflammation were more vivid in this situation, and the quantity of lymph and purulent matter thrown out was greater than in the other parts. It was evident that the intestinal tunics were not all destroyed in an equal manner, *the destruction of the peritoneal being considerably greater than that of the mucous.* On the same portion of intestine, and within an inch and a half of the perforation, a small spot of ulceration was discovered, *confined entirely to the serous membrane.* The mu-

cous coat was throughout healthy, and even in the immediate vicinity of the perforation, showed no traces of increased vascular action. We examined most carefully the whole intestinal tract, from the stomach to the rectum, but could discover no further lesion. The mesenteric glands were of their normal volume. The case I consider to be almost unique.—*Edin. Monthly Journal.*

VEGETABLE ORIGIN OF PORRIGO DECALVANS.

Porrigo decalvans is a disease of the skin, usually of the scalp, producing a falling-off of the hair from the parts affected. It is characterised by rounded spots or patches, the surface of which is usually covered with small dry scales, or a white bran-like powder. If this powder be examined with the microscope, it will be found to consist of minute *cryptogamic* plants. The hairs that have fallen off are observed to be encrusted on all sides with these singular growths, and to form a veritable vegetable sheath, which invests them from their point of emergence from the skin to the extent of three or four millimetres. M. Gruby (the discoverer, we believe, of this pathological curiosity) has denominated the fungus by the appellation of *microsporon Andouini*, in compliment to M. Andouin, whose inquiries have done so much to illustrate the nature of the parasitic plants, which infest the tissues of living animals.

This porriginous fungus commences its development on the surface of the hairs, at the distance of one or two *millimetres* from the epidermis; the first sign of its existence being that the tissue of the hair is observed to lose its transparency, in consequence of the formation of excessively minute molecules on its surface.

The altered tissue exhibits the appearance of having distinct fibres, and of cells larger than the fibres of the hairs, elongated and situated parallel to their axes. It is in this part that the *microsporon* is first perceived. By its gradually extending in all directions, the adjoining hairs are quickly affected in a similar manner, and the morbid growth advances until patches of the affected hair fall off, and leave the skin nearly quite bald.

These fungi are developed and multiplied with surprising rapidity, and hence the extension of the diseased patches is sometimes very sudden. The hairs usually break off at the point where they are invested with the vegetable covering. The thickest hairs resist the invasion of the disease the longest. The vegetable nature of *porrigo decalvans* is a fact that speaks for the contagiousness of the disease. How far any of the other varieties of this scalp-affection are traceable to a similar origin has not yet been ascertained. The first step to the cure of a disease being to understand its nature, we may fairly anticipate that the discovery of M. Gruby will have a therapeutic, as well as a physiological, interest.—*Med. Chir. Rev.*, Jan.

*** We have recently been enabled to confirm the observations of M. Gruby. Porrigo decalvans is, in many cases, very intractable, but we have found it yield to local applications, in some instances, of sulphate of iron. Others we have treated successfully by tincture of cantharides, combined with castor oil.

BRITISH MEDICAL JOURNALS.

In another page (556) of this week's Lancet will be found an analysis of a case (originally published in the London and Edin. Monthly Journal), which is not very often met with, viz., one of *ulceration of the peritoneum*. Serous membranes resist this action in a remarkable manner, and rarely become, *in the first instance*, the seat of the destructive process in question. Some few cases, however, are recorded by Scoutetten, Portal, and other authors. Dr. Stokes has related an instance in which the pleura was the seat of ulceration. These, taken in connection with the present well-marked example, seem to us to place the fact of its occurrence beyond all doubt.

Serous membranes do not differ essentially, in their structure, from the cutaneous and mucous textures. All are composed of a layer of extravascular, epidermic, or epithelial scales, placed over a vascular network. When serous membranes become the seat of inflammation, lymph, a highly-organisable substance, is produced. When cutaneous, or mucous tissues, on the other hand, are inflamed, a less organisable matter is effused, and ulceration is a frequent result. Let us suppose, then, this organisable matter to be absent, owing to the extremely acute character of the sub-serous inflammation, destroying the surface which should yield it. Or let us have this matter less capable of organisation than usual (not a very unfrequent occurrence), and what may we expect to find? Nothing more or less than what the present case shows us, namely, abrasion, or ulceration, of the peritoneal, or of any similar serous surface. Dr. Adams' remarks on this case are applicable and not too extended. The style, however, of his communication is not particularly clear, and we could wish that Dr Adams had rendered the case complete by describing the appearance presented by the ulcerated surface.

Ergot of Rye in Retention of Urine.—The following is an outline of a case related

in the same journal by Dr. Ross, of Cambrismore:—A man, ætat. 72, was seized with retention of urine, which continued for three days. Incontinence then took place; the urine dribbled away, and a catheter being introduced, a large quantity, still remaining, was drawn off. Retention again required the use of the catheter, and different remedies were applied for two months, when Dr. Ross resolved administering ergot of rye, the use of which he had seen mentioned in one of the periodicals. The medicine was given in ten-grain doses every morning, and increased to half a drachm. Great irritability of the bladder was the result. The effects of the medicine were still kept up, and after two months' perseverance the expulsive power of the bladder was completely restored. Such being the facts of the case the remedy seems to merit a trial, "especially where the retention occurs in old subjects, and depends on paralysis of the bladder, arising simply from previous over-distention."

Sex of the Child and the Safety of Parturition.—Professor Simpson recently made a statistical communication to the Medico-Chirurgical Society of Edinburgh, for the purpose of proving that the sex of the child has a very marked and demonstrable influence upon the safety of the process of parturition, in regard both to the mother and infant. This was due to the slight but decided difference of size in the heads of male and female children.—*Edin. Monthly Jour.*

In the "Medical Gazette" of June 13th, the subject of pneumo-thorax (?) is continued by Dr. Hughes. The diagnosis, prognosis, and treatment, are considered, but we discover nothing in the paper which is not already to be found in the ordinary text-books. Dr. Hughes has made a collection of cases, the illustrations afforded by which form the chief merit of the essay.

The following tables present examples:—

"Of sixty-two cases of pneumo-thorax there were—

Males	48	
Females	14	62

The left side was affected in—

Males	28	36
Females	8	

The right side was affected in—

Males	20	26
Females	6	
		62

The age of the patients was under 20 in—

Males	8	9
Female	1	

The age of the patients was 20, and below 30, in—

Males	19	26
Females	7	

The age of the patients was 30, and below 40, in—

Males	11	16
Females	5	

The age of the patients was 40, and under 50, in—

Males	6	7
Female	1	

The age of the patients was above 50, in—

Males	3	62."
Was not mentioned	1 male	

"Duration of life in sixty-two cases of pneumo-thorax:—

"A few hours"	3
Less than twelve hours	2
Twelve hours, and less than forty-eight hours	4
Forty-eight hours, and less than one week	10
"Some," "several," or "many" weeks	5
One week, and less than one month	4
One month, and less than three months	5
Three months, and less than one year	8
Twelve months (at least, Dr. Houghton)	1
Thirteen months (Dr. Stokes)	1
Three years (at least, Dr. Barlow)	1
Uncertain, or unrecorded	18
	62"

Ischuria Renalis.—In the same journal Dr. Theophilus Thompson relates a case under this title in which "urine was not secreted for the space of about 120 hours. During the subsequent forty-eight hours, only half an ounce was passed, and the natural quantity was not restored till after the expiration of a month from the period of its interruption; nevertheless, no urgent cerebral symptoms at any time occurred."

Dr. Thompson believes that he succeeded in restoring the secretion by applying nitrate of silver to the loins. He thus, at least, endeavoured to renew an issue which had been a few days previously suppressed. The case is very badly related, and were the subject worthy of the trouble we should have no difficulty in convincing the author that deficiency in details, and a want of correspondence between the phenomena observed (?) during life, and the appearances after death can scarcely be compensated for by far-sought quotations, and such high-sounding names as Valisneri and Marangoni.

A few pages further on is a case of malformation, described by Dr. Starr, of Kettering. It is one of "Non-Occlusion of the Urachus," and occurred in a female child. "From the period of its birth, thirteen weeks previously, the urine had escaped at the umbilicus, the urethra being at the same

time perfect. The excretion passed through the natural channel, and probably simultaneously with its evacuation from the navel, as the flow of it from the latter situation was not continual, but occurred at intervals, as if it were regulated by the detrusive action of the bladder and muscles associated therewith. The umbilicus, in its general appearance, was larger and more open than usual; and in the centre of the cartilaginous nipple-like projection I found an orifice which admitted of an ordinary-sized probe. This was easily passed in the direction of the linea alba towards the bladder. It was clear to me, from a consideration of all the circumstances, that the common ligamentous band which passes from the fundus of the bladder upwards to the umbilical cord, and known by the name of urachus, had assumed, and retained subsequently to birth, the form of a duct, and this duct opened into the bladder. In the calf, and other quadrupeds, a communication resembling that which I have described always exists; but, to use a quotation from that distinguished anatomist, M. H. Cloquet, "in man, it (the urachus) rarely forms a canal, excepting in cases where the urethra is wanting."

In the "Dublin Medical Press" for Jan. 10th, Mr. Neilson, of Killala, describes a case of complete lateral dislocation of the bones of the forearm (we should add, at the elbow-joint) as follows:—

"A lad, fourteen years of age, fell on the street from the seat of a jaunting-car. Surgeon Townley, of this town, and I saw him, together, about two hours after the accident. The right elbow-joint appeared nearly double its natural breadth. The coronoid process of the ulna was resting *outside* the external condyle of the humerus; the condyle itself was broken and very moveable; the olecranon was drawn up nearly three inches from its proper position. The olecranon and coronoid processes could be very easily felt. The head of the radius was distant from the humerus the entire thickness of the head of the ulna, and perfectly moveable; the separation of the articulating surfaces was so complete that the arm could be flexed or extended considerably, and the hand placed prone or supine with great ease, and with very little pain. The motion of the hand was so very easy, that with my fingers acting on the head of the radius, the hand could be moved. When the arm was flexed the olecranon, of course, projected very much behind the humerus, while the coronoid process came rather in front of that bone. There was no external wound, but the integuments were much stretched.

There was some difficulty at first in reduction, I think in consequence of the fractured portion of the condyle coming in the way of the ulna; but by keeping it steady, making first slight extension, and then gently flexing the arm, at the same time pressing the bones of the arm and forearm in opposite directions, the dislocation was easily reduced. A bandage was applied with splints resting on each condyle of the humerus, and the parts were kept constantly wetted with a lotion of spirits and vinegar. There was very little swelling, and even that has now almost entirely disappeared. There has been no fever.

Mr. Neilson remarks that about a year and a half since, the same arm was dislocated backwards. He imagines that similar injury was produced now in the *first* instance, and that the fracture of the condyle and the lateral dislocation were *secondary*, being produced by the weight of the body coming on the injured joint, and the broken condyle more readily permitting the coronoid process of the ulna to be driven external to the humerus."

The "Provincial Journal" contains a series of quotations and remarks by Mr. Bower Harrison, on the origin of catarrhs. He concludes "that wet and cold are not to be considered as the only sources of catarrh." He thinks it may be inferred that if the body be previously in good health, if the animal heat be kept up by exercise and sufficient clothing, and if there be a free change of air, there will not be so much danger as is imagined, even from sudden transitions of temperature. And "certainly many of the effects attributed to cold and moisture arise, in reality, from the crowding together of people in ill-ventilated rooms."

The subject has been cleverly treated, but the paper abounds in unquotable quotations. In the same number some cases in confirmation of "the contagious nature of puerperal fever, and of its connection with erysipelas," are detailed by Mr. F. Elkington, of Birmingham. The facts correspond with the heading, which is all that can be said here on the matter.

NEWS OF THE WEEK.

It is now some ten days since we were assured, on good authority, that several important deviations from fact were contained in the first notice we published of the proceedings of Mr. Wakley, Coroner for Middlesex, in reference to the proposed sale of effects stated to be the property of Lieutenant Munro, of the Royal Horse Guards. We lost no time in making the most careful inquiry into the accuracy of the statements in question, and, with the readiness with which we are always anxious to admit even an unintentional error, we feel bound to say that very serious misrepresentations did occur in the account that was furnished to

us. So far from there having been any unnecessary harshness on the part of Mr. Wakley in reference to the sale in question, the result of our investigation convinces us that the conduct of that gentleman was marked throughout by the most scrupulous delicacy.—*Morning Post*, Jan. 18, 1844.

THE deaths in the metropolis for the week ending Jan. 6, 1844, show a decrease of 64, as compared with the previous week, the total having been 933 (*vice* 1007). Diseases of the lungs, 285 (*v.* 290); epidemic and contagious diseases, 176 (*v.* 196); diseases of the brain and nervous system, 168 (*v.* 157); cancer, dropsy, and diseases of uncertain seat, 108 (*v.* 109); diseases of the stomach and liver, 56; old age, 54; diseases of the heart and arteries, 25; deaths from violence, cold, starvation, 19; diseases of the kidneys, &c., 9; deaths from childbirth, &c., 8; rheumatism and diseases of bones, 5; diseases of the skin, 2; causes of death not specified, 18.

The district of Wandsworth and Clapham has been introduced for the first time in the above weekly return. The population of this district in 1841 was 39,855, and it occupies a surface of 10,800 acres. Its weekly average of deaths during the last five years has been 14. The number of deaths returned for the week ending Saturday, the 6th of January, 1844, amounted to 15. The cause of the deaths from the district of St. Peter's, Walworth, 14 in number, not having been stated, they have been added to the head of *causes not specified*.

Of 16 persons who died of small-pox, 12 had not been vaccinated. In two cases the circumstance is not stated.

INTERVIEWS AT THE OFFICE.

COMPLAINTS having been repeatedly made to us of the uncertainty which has attended the endeavours of our correspondents to obtain interviews with some person who could exercise authority in the editorial department of this Journal, we embrace this opportunity of stating that, in future, the

SUB-EDITOR OF THE LANCET

WILL ATTEND AT THE OFFICE, PRINCES-STREET,

On Mondays and Wednesdays,

From the hours of ONE to THREE o'clock.

It is particularly requested that all *communications* and *articles* which are forwarded for *publication* in THE LANCET, be sent to the EDITOR, at the OFFICE; and that letters of a private or personal character, which may refer, in any manner, to the proceedings of the Journal, be addressed to the EDITOR, at his residence, in BEDFORD-SQUARE.

APOTHECARIES' HALL, LONDON.

NAMES of gentlemen who received certificates of qualification to practise as apothecaries on Thursday, January 11, 1844:—Charles Price Symonds, Great Yarmouth; Edward John Wright, London; George Hammond, Dover.

CORRESPONDENTS.

Amicus should not have attended to the "verbal apprisal" of the town-servant. In all such cases he should request to be served with the "order" that is formally described in the Act of Parliament. The verdict was most absurd. The inquiry itself was demanded by *the law*. Its utility is to be found in the object under which the law, once for all, declared that in such cases a judicial investigation should be instituted. There does not appear to have existed any suspicion that the death was produced by violent means, and the *history* of the case probably justified the jury in considering the cause to have been purely natural. Our correspondent does not seem to have considered that it was any other.

Syntax.—The law (in the form of orders and regulations) is in the making of the poor-law commissioners, who may retain or remove the surgeon as they think fit. Representations and questions on the case should be addressed to them, at Somerset-house.

We have received the communication from *Dr. Williams*, of Colchester, containing Boyden's letter, as also the letter of the 13th inst., but must again decline taking any further part in the controversy, which, if continued at all, must be continued locally.

A. B. C.—We agree with our correspondent in pronouncing the advertisement alluded to objectionable, both in form and substance, and shall not allow it again to appear on the cover of THE LANCET.

We are sorry we cannot insert *Mr. Dennis's* communication, which we have returned by post to that gentleman.

The cases of *Mr. Corfe* have been again unavoidably postponed, but shall appear without fail next week.

Dr. Marshall Hall's practical observations have been received, and shall be inserted in an early number.

We have received *Mr. Harvey's* letter, and regret, with him, that Mr. Walne did not take a part in the discussion at the Medico-Chirurgical Society, the report of which appears in the present number.

Communications have been received from *Mr. Leigh*, *Dr. Ryan*, *Justus*, *Mr. Roch*, *Dr. Witherfield*, *Mr. J. Y. Hunter*, *Mr. G. Ross*, *Mr. T. Hunley*, and *Mr. E. T. Wait*, and shall be duly attended to.

ERRATA.—In page 486, col. 1, 4th line from top, for "outward," read *onward*.—In page 528, col. 1, a few lines from bottom, for "Dr. E. Edwards," read *Dr. E. Williams*.